AN

EXPERIMENTAL INQUIRY

INTO THE

LAWS OF THE VITAL FUNCTIONS.

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INTO THE

LAWS OF THE VITAL FUNCTIONS;

WITH A VIEW TO

REMOVE THE INCONSISTENCIES OF OUR PRESENT DOCTRINES,

AND

THUS TO ESTABLISH MORE CORRECT PRINCIPLES

RESPECTING THE

NATURE AND TREATMENT

OF

THEIR DISEASED STATES.

BY

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FOURTH EDITION,

GREATLY ENLARGED BOTH IN THE PHYSIOLOGICAL AND PRACTICAL PART.

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PREFACE

TO

THE FOURTH EDITION.

Even the least reflecting of our profession will admit, that the better we understand the laws of our frame, the more successful will be the treatment of its diseases.

When a new medicine is proved to be capable of relieving a disease which has hitherto defied our means, the value of the fact is at once apparent; and the trouble of acquiring a knowledge of it, and applying that knowledge, very trifling. It is, therefore, generally and thankfully adopted.

But although it will be admitted by all, that the correction of an old or the adoption of a more successful principle of treatment, may be more valuable than the discovery of any single remedy; it can never be received or applied with the same readiness. In a profession which teems with so many proposed improvements, not one in a hundred of which proves of any value, it is necessarily long before any suggestion, more complicated than a simple matter of fact, finds its due level; and however easy the knowledge and application of a new remedy, the adoption of a new principle of treatment is in all instances more or less difficult and laborious to those who have long reasoned on different principles.

Many additions have been made to the physiological department of the present Inquiry since the publication of the last edition, which the Royal Society have done me the honour to publish in eight papers during that interval.

In the present volume, these additions are incorporated with those parts of the Inquiry to which they relate; and the greater and most important part of the practical department is now for the first time offered to the public; for at the time of the publication of the last edition, my views not being sufficiently matured respecting the more complicated part of the subject, I abstained from entering on it.

The chief object of the former editions of the present Inquiry was to remove the inconsistencies which prevailed in our doctrines respecting the general laws of the Vital Functions. The facts by which this, as far as I am capable of judging, has been effected,[1] have now been generally admitted; in consequence of many of the most important experiments on which they are founded having been publicly repeated with the same results both in London[2] and Paris.[3]

The chief object of the present edition is the application of these results to improve the practical department of our profes-

[1] Philosophical Transactions for 1836. The Royal Society has for the last four-and-twenty years, from time to time, published the results of the investigation as it proceeded, and in 1836, although in opposition to its general practice, did me the honour to republish a concise statement of the whole of those results which had previously appeared in its Transactions, for the purpose of bringing them into one view, in order that the necessary inferences from them, when an opportunity is afforded of comparing together the various positions arrived at, might be apparent.

[2] The Philosophical Transactions, and the Journals of the Royal Institution, both for the year 1822.

[3] De l'Influence du Système Nerveux sur la Digestion Stomacale; par MM. Breschet, D.M.P., Chef de Travaux Anatomiques de la Faculté de Médecine de Paris, etc.; H. Milne-Edwards, D.M.P.; et Vavasseur, D.M.P. (Mémoire lu à la Société Philomatique, le 2d Août, 1823,) Extrait des *Archives Générales de Médecine*, Août, 1823.

sion, by laying before the reader at one view, the observations which have gradually accumulated during a practice of long continuance, in confirmation of the advantages thence arising; and this I do with the more confidence, that I can now refer to the experience of many of my professional brethren in support of those observations.

IT will be found from what I shall have occasion to say in the Third Part of the present edition, that the means by which the removal of the inconsistencies, which clogged our knowledge of the general laws of our frame, influence the treatment of disease; are referable to the three following heads, each of which it will be my object in that Part fully to illustrate.

1. Our not being sufficiently aware, from a defective knowledge of the relation the various functions bear to each other, of the tendencies of continued states of chronic disease; in consequence of which, in a numerous class of such diseases, the curative stages are often allowed to pass disregarded.

2. The attention, in many acute diseases, from the same cause being often confined to the more evident train of symptoms; where the derangement has had its origin in a more obscure affection, without the removal of which that of the more prominent disease is impossible; and

3. From our not having been aware either of the nature or most important functions of the nervous influence, or the seat of the organs, on which these functions immediately depend; we have neither been aware of many of the symptoms which indicate the failure of that influence, nor in possession of the most effectual means of remedying its effects.

It is reasonable to conclude that a knowledge of the nature of what we shall find to be the leading power in the vital Functions, and the seat of the organs which supply and of those which convey and apply it, must under many circumstances essentially influence the treatment of their diseased states.

In the following volume, as in all the former editions of it, I have confined myself to a statement of facts and their necessary inferences. The attempt at complete systems of the animal functions has been the bane of physiology. In its early stages, the whole science, if it deserved the name, was little better than fanciful; and in our present greatly improved systems, enough of hypothesis is often retained to make it impossible for the learner to distinguish what is well founded; and thus uncertainty is spread over the whole. It has constantly been my aim to separate what we know from what we do not, the true from the false and the uncertain, to admit of no inference but such as is supported by direct evidence, and where the inference seems so probable as to deserve mention, but not certain, to state it as such. By this method we shall not soon arrive at a complete system, but it is the only one by which a regular advance towards such a system can be made.

My object in the following Inquiry, as it has been from the commencement of my physiological labours, is confined to the general laws of the animal economy; but comprehends the whole of those laws: and my attention has been uniformly directed to the correction of the inconsistencies which prevail respecting them, and the errors in the practical department of our profession which are their necessary consequences.

With regard to more minute physiological investigations, namely, those which have for their object the laws which are peculiar to the functions of individual organs, it is a far more complicated subject; necessarily requires the joint efforts of many labourers, and rests on much more extensive and varied foundations; of which a knowledge of the general laws of our frame is but one: but as the functions of all our organs partake of those laws, a knowledge of them is the first step towards that of its individual functions.

It is this part of these foundations alone which I have attempted to lay, and as the accuracy of the experiments on which it rests,

is now confirmed by the testimony of many of the first physiologists both of this and other countries, the reader has only to judge of the accuracy of what I believe to be the necessary inferences from them; the most gratifying argument in favour of which is the more successful treatment of an extensive class of diseases to which these inferences have led; the proofs of which he will find in the Third Part of the present volume.

It will be evident to the reader that it is impossible for me, in such a work as the present, to enter at length either into the phenomena or treatment of all the forms of disease to which I have occasion to refer. My object is only to point out the manner in which the peculiar nature of those diseases makes it necessary to regulate the treatment, and particularly in the more complicated cases, taking the most important instances as illustrative of the whole.

FROM the commencement of the Investigation, most of my experiments were made on the newly-dead animal. In this, it will appear, there are other advantages beside saving the sufferings of the animal. What we call death, we shall find, is the ceasing of the sensorial functions, the whole of the vital functions, with the exception of respiration, remaining.

CONTENTS.

PART I.

Page

ON THE STATE OF OUR KNOWLEDGE RESPECTING THE GENERAL LAWS OF THE ANIMAL ECONOMY AT THE TIME THE INVESTIGATION DETAILED IN THE SECOND PART OF THIS VOLUME WAS COMMENCED - - - - - - - - 3

CHAPTER I.

The Report made to the Class of Physical and Mathematical Sciences of the Institute of France on the work of M. le Gallois, entitled "Expériences sur le Principe de la Vie, notamment sur celui des Mouvemens du Cœur, et sur le Siége de ce Principe" - - - - - - - - 3

CHAPTER II.

Observations on the foregoing Report - - - - - 26

PART II.

EXPERIMENTS MADE WITH A VIEW TO ASCERTAIN THE GENERAL LAWS OF THE FUNCTIONS OF LIFE IN THE MORE PERFECT ANIMALS, AND THE PHYSIOLOGICAL INFERENCES TO WHICH THEY LEAD - - - - - - - 35

CHAPTER I.

On the Principle on which the Action of the Heart and Vessels of Circulation depends - - - - - - - 36

CHAPTER II.

Page

On the Relation which subsists between the Heart and Vessels of Circulation and the Nervous System - - - - - 43

CHAPTER III.

On the Principle on which the Action of the Muscles of Voluntary Motion depends, and the Relation which they bear to the Nervous System - - - - - - - - 55

CHAPTER IV.

On the comparative Effects of Stimulants, applied to the Brain and Spinal Marrow, on the Heart and Muscles of Voluntary Motion - - - - - - - - - - 60

CHAPTER V.

On the Sources and Nature of the Powers of Circulation - 69

CHAPTER VI.

Some additional Facts and Observations respecting the Relation which subsists between the Nervous and Muscular Systems in the more perfect Animals, and particularly respecting the Functions of the Ganglions - - - - - - 79

CHAPTER VII.

On the Principle on which the Action of the Vessels of Secretion and Assimilation depends, and the Relation which they bear to the Nervous System - - - - - - 93

CHAPTER VIII.

On the Effects on the Lungs of removing a portion of each of the eighth pair of Nerves - - - - - - 100

CHAPTER IX.

On the Principle on which the Action of the Alimentary Canal depends - - - - - - - - - - 102

CHAPTER X.

Page

On the Relation which the Alimentary Canal bears to the Nervous System - - - - - - - - 103

CHAPTER XI.

On the Process of Digestion - - - - - - 104

CHAPTER XII.

On the Effects on the Stomach and Lungs of destroying certain Portions of the Spinal Marrow, compared with those of removing a part of one or both of the Eighth Pair of Nerves 114

CHAPTER XIII.

On the Cause of Animal Temperature - - - - - 122

CHAPTER XIV.

Some additional Observations relating to the Function of Digestion - - - - - - - - - - 124

CHAPTER XV.

A Review of the Functions of the Nervous System, and of the Relation which they bear to the other Functions of the Living Animal - - - - - - - - 129

CHAPTER XVI.

On the Sensorial Functions - - - - - - 140

CHAPTER XVII.

On the Relations which the Vital Functions bear to each other, and the manner in which they partake of the Sensorial Functions - - - - - - - - - - 143

CHAPTER XIX.

On the Nature of the Vital Powers - - - - - 164

CHAPTER XX.

Page
On the Nature of Sleep - - - - - - - 192

CHAPTER XXI.

On the Nature of Death - - - - - - - 209

CHAPTER XXII.

On the means by which the Frame of the more perfect Animal is formed into a whole - - - - - - 254

PART III.

THE APPLICATION OF THE INFERENCES FROM THE FOREGOING INVESTIGATION, WITH A VIEW TO IMPROVE OUR KNOWLEDGE OF THE NATURE AND TREATMENT OF DISEASE - 268

Introduction - - - - - - - - - 268

CHAPTER I.

On the Diseases of the Sanguiferous System - - - 271
Sect. I.—On Inflammation - - - - - - 272
Sect. II.—On Congestion - - - - - - 281

CHAPTER II.

On the Diseases of the Nervous System - - - - 285
Sect. I.—On the Diseases of the Brain - - - - 286
1. On Nervous Apoplexy - - - - 287
2. On Suspended Animation - - - 296
Sect. II.—On the Diseases of the Spinal Marrow - - 298

Page

Sect. III.—On the Diseases which have their seat equally in the Brain and Spinal Marrow - - - - - - - 303

On the Nature and Phenomena of the Sympathy of Parts in the more Perfect Living Animal - - - - 312

On the Process by which Organic Disease is established - 319

1. On the Diseases of the Central Organs of the Vital System, when the offending cause is seated in these organs themselves - - - - - - - 324

2. On the Diseases of the Central Organs of the Vital System, when the offending cause makes its impression on those of the Sensitive System - - - 337

3. On the Diseases of the Central Organs of the Vital System, when the offending cause makes its impression on a distant part of that System - - 339

On the States which precede Debility of the Central Organs of the Vital Sytem, the most common form of which has obtained the name of Bilious Complaints - - - - - - 349

On the first stage of the Debility of the Central Organs of the Vital System belonging to the third division of this Section - - - 353

On the last stage of the Debility of the Central Organs of the Vital System belonging to the third division of this Section - - - 354

On the Treatment of the first stage of the Debility of the Central Organs of the Vital System belonging to the third division of the third Section - - - - - - - 364

Sect. IV.—On a certain state of Dyspepsia and habitual Asthma 370

CORRIGENDA.

Page 80, line 19, *for* part, *read* parts.
83, last line of page, *dele* the reference at the end of the note.
97, line 4, *after* is, *insert* often.
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167, line 8 from bottom, *for* him, *read* me.
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257, line 16, *before* muscular, *insert* sensorial and : and *for* power, *read* powers.
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AN INQUIRY

INTO THE

LAWS OF THE VITAL FUNCTIONS,

&c. &c.

Both the animal and vegetable world differ from inanimate matter, in affording a peculiar class of results when impressed either by mechanical or chemical agents. The quality on which the peculiarity of these results depends is called life. It is a state of which certain properties are the result, and it is essential that its name should convey this fact and no more.

In the present edition the author will use the term life, in the sense here defined, instead of vital principle, which has been employed so vaguely, that although he has repeatedly defined the sense in which he uses it, he has found himself very frequently misunderstood where it occurs.

The phenomena of life are more varied, and have been observed and arranged with more care in the animal than the vegetable world. To the former the present Treatise is confined.

It is divided into three Parts. In the first Part the reader will be made acquainted with the state of our knowledge on the subject

at the time the following investigation was commenced. In the second I shall detail the experiments, and point out the inferences to which they individually lead; and this Part will conclude with a general review of the physiological results, and their necessary inferences. In the last I shall point out the application of those inferences, with a view to improve the practical department of our profession, and state the practical results from the experience of many years.

PART I.

ON THE STATE OF OUR KNOWLEDGE RESPECTING THE GENERAL LAWS OF THE ANIMAL ECONOMY AT THE TIME THE INVESTIGATION DETAILED IN THE SECOND PART OF THIS VOLUME WAS COMMENCED.

The object of this Part cannot, I think, be better accomplished than by laying before the reader a translation of the Report of the Committee of the Institute of France, on the Experiments of M. le Gallois, abridged in the less important parts; and such observations on it as it appears to demand.

CHAPTER I.

The Report made to the Class of Physical and Mathematical Sciences of the Institute of France on the work of M. le Gallois, entitled "Expériences sur le Principe de la Vie notamment sur celui des Mouvemens du Cœur, et sur le Siége de ce Principe."

The Class having charged M. de Humboldt, M. Halle, and myself,[1] to make a report to it on the Memoir read at a meeting of the 3d of June last, by M. le Gallois, Doctor of Medicine, respecting the nature of the power of the heart, and the source whence it derives its power,[2] we are about to present to it a

[1] M. Percy.

[2] "Concernant le Principe des Forces du Cœur, et le Siége de ce Principe."

detail which will, perhaps, be as long as the Memoir itself, because, without the necessary details and explanations, it would be impossible to appreciate all the merit of this excellent work.

It was not till after the circulation of the blood was discovered by Harvey, early in the seventeenth century, that physiologists turned their attention to the cause and mechanism of the movements of the heart, which have, since that time, given rise to so many different systems.

We shall not speak of those of Descartes,[1] of Sylvius de la Boe,[2] of Borelli.[3] They are very absurd, and serve only to prove how unfortunate were the first attempts to explain one of the most important functions of the animal economy. We shall begin with the distinction which Willis first pointed out between the nerves destined for the voluntary, and those for the involuntary, motions.

He placed the origin of the latter in the cerebellum or little brain, of the former in the brain properly so called. He taught that the motions of the heart, and other vital organs, experience no interruption, because the cerebellum is in a state of constant activity; but that the organs of voluntary motion, on the contrary, require repose, because the brain acts only by intervals.[4] This distinction was very generally admitted till the middle of the last century.

It was chiefly with a view to it that the division of the eighth pair of nerves,[5] from which it was maintained that almost all the nerves of the heart proceed, was performed in different

[1] L'Homme de René Descartes, et la Formation du Fœtus, avec les Remarques de Louis Laforgue, Paris 1677, p. 4 and 106.

[2] Francisci de la Boe Sylvii Opera Medica, Genevæ, 1681, p. 5, 27, 28, 33, 475.

[3] Joh. Alph. Borelli de Motu Animalium. Hagæ Comitum, 1734, p. 89—92.

[4] Tho. Willis Opera Omnia, edente Ger. Balsio, Amstelodami, 1682, tom. i., de Cerebri Anatome, cap. xv. p. 50.

[5] See the Account of the Nervous System in the Preface.

countries. The object was to prove that it is from the cerebellum that the heart derives all its power, and it was alleged that the animal died in this experiment, in consequence of the communication between these organs being interrupted. But, besides that it dies too slowly to permit us to ascribe its death to this cause, it has been proved in later times by several philosophers, and particularly by M. le Gallois, in a memoir which the Class ordered to be inserted in the transactions of learned correspondents, that death here proceeds from quite a different cause. It has sometimes happened, indeed, that animals have died almost suddenly after the division of the nerves in question, and the partisans of Willis have not failed to lay much stress on this circumstance, of which their adversaries could give no satisfactory explanation. But M. le Gallois has demonstrated, in the memoir to which we have just alluded, that sudden death in this case only happens in certain kinds of animals, and in these only when they are very young, and that it is the effect of suffocation,[1] more or less complete, from the closing of the upper part of the windpipe. There is nothing then in these facts in favour of Willis; to which we may add, that the eighth pair of nerves does not arise from the cerebellum, and that most of the nerves of the heart do not belong to this pair.

Boerhaave was of the same opinion with Willis; but, besides the power of the nerves, he admitted two other causes of the motions of the heart; the action of the blood of the arteries of the heart on its fibres, and of the venous blood on the surface of its cavities. According to him, the concurrence of these three causes produces the contraction; and the simultaneous interruption of their action, in consequence of the contraction, gives rise to the dilatation, during which their action is renewed.[2] But this explanation, with the exception of what regards the stimu-

[1] Asphixie. This I translate suffocation, because we use the term Asphixia in a very different sense. Culleni Synopsis Nos. Method. Gen. 44.

[2] Her. Boerhaave Instit. Medicæ, § 409.—Van Swieten in Aphorismos, &c. Lugduni Batav. 1745, tom. ii., p. 18.

lant effect of the blood on the internal surface of the heart, is contradicted by fact, which has not prevented its reception in the schools, with another error that has made no less noise.

We allude to Stahl, and his presiding spirit, or Archæus, as he calls it, which was supposed by him to regulate all the movements of the living body, subjecting them to the will, or rendering them independent of it, according as they are merely useful, or absolutely necessary to life, and presiding, above all, over those of the heart, and, through the influence of the nerves, insuring their continuance; a species of reverie which is inconsistent with all the true principles of physiology.

After all, where would the Stahlians place this simple and indivisible being? In the brain without doubt. But then, how does it happen that an animal may live, and the motion of its heart continue after it is decapitated? Would they place it in the heart itself? But all animals, and especially those of cold blood, live a longer or shorter time after the heart is removed.[1]

Other writers, such as Abraham Eus,[2] Stœhelin,[3] &c., have also endeavoured to explain the motions of the heart; but their systems, almost as soon forgotten as conceived, do not deserve to detain us.

Those of Boerhaave and Stahl reigned almost alone, when, in 1752, Haller published his experiments on irritability. These experiments and those of his followers tend to prove, that the contractile power belongs essentially to the muscular fibre. That property which Haller sometimes speaks of under the name of *vis insita*, sometimes after Glisson, under that of irritability, is the source of all the motions which take place in the animal; but it cannot produce them, except some cause, some stimulant, determines it to act. Thus all muscular motion implies two

[1] For an exposition and refutation of this system, see Haller's Element. Physiolog. tom. i. p. 480—8, and tom. iv. p. 517—34.

[2] Dissertatio Physiol. de Causa vices Cordis alternas producente. Ludg. Batav. 1745.

[3] Dissertatio de Pulsibus. Basileæ, 1749.

things,—the irritability on which the contraction of the muscle depends, and the stimulant which determines the irritability to act. The irritability is everywhere the same. It only varies in intensity in the different muscles; but it does not obey the same stimulants in all the muscles. The nervous power is the natural stimulant to all which are under the influence of the will; and it is by exciting or suspending the action of that power on the irritability of such or such muscles, that the will causes any particular part to act or to be at rest. It is not thus with the muscles of involuntary motion; these are affected by stimulants of different kinds, which are appropriated to their different functions, and altogether different from the nervous power. It is the blood which is the natural stimulant of the irritability of the heart: alimentary substances, of that of the intestinal canal, &c.

We easily deduce from these principles the explanation of the leading circumstances which we observe in the motions of the heart. Thus its motions are involuntary, because they are independent of the nervous system; they take place without interruption during life, because the irritability which produces them belongs essentially to the fibres of the heart, and the blood which excites them is constantly supplied to this organ by the veins as it is carried off by the arteries. The contraction and dilatation succeed each other alternately and regularly, because the blood always occasions the former both in the auricles and ventricles, and the contraction itself, by expelling the stimulant, occasions the dilatation, which renews the contraction by allowing access to new blood.

Such is a summary view of the celebrated Hallerian theory of irritability. That theory was not contrived in the closet, like the others of which I have spoken: it was founded, as we have said, on experiments made by Haller himself, and by the most distinguished of his scholars, who then occupied, or have since occupied, the first rank among the anatomists and physicians of the last age. The inferences from these experiments, which were repeated throughout Europe, found almost everywhere supporters;

but they found also some opponents of the greatest reputation. The principal cause of this difference of opinion, and that respecting which authors have not yet been able to come to any agreement, is the question, whether the motions of the heart are really independent of the nervous system?

We may reduce to three heads the facts by which the school of Haller has supported the affirmative. 1st. If we interrupt all communication between the heart and the brain, *the only source of nervous power*, by dividing the nerves which go to the heart, the spinal marrow in the neck, or even decapitation, the motions of the heart continue as before. 2d. If we cut out the heart and place it on a table, it continues to beat, and sometimes for a long time. M. de Humboldt has shown that it beats more strongly, and for a longer time, when it is suspended. 3d. We always produce convulsions, even for some time after death, in the muscles of voluntary motion, by irritating their nerves, either mechanically or in any other way. On the contrary, the irritation of the nerves of the heart neither occasions a change in its motions, nor recals them when they have ceased. The same observation is true of the medulla oblongata and spinal marrow, the irritation of which occasions strong general convulsions, but produces no effect upon the heart.

These facts are correct, except perhaps those of the third head, respecting which there is some difference of opinion. For, in admitting them, the adversaries of irritability have asked, Why, if the nervous power has no action on the heart, is this organ supplied with nerves, and why is it so evidently subjected to the influence of the passions? Haller never gave any satisfactory explanation of these objections, but everything proves that he felt all their force. When we read with attention all that he has said of the motions of the heart, in his dissertations on irritability,[1] and, above all, in his great work on Physiology,[2] we are

[1] Mémoires sur la Nature Sensible et Irritable des Parties, etc., Lausanne, 1756.—Opera Minora, tom. i.

[2] Element. Physiol. lib. iv. sect. 5, et lib. xi. sect. 3.

struck with the contradictions which we meet with in them, and which make the perusal of them fatiguing. Through all of them his great object is to prove, that the motions of the heart are independent of the nervous system. All the facts, all the experiments, all the observations that he brings forward, tend to this end; and yet he seems to admit in several places that the nerves possess an influence over the heart. It is true that it is with an air of doubt that he admits it, and confines himself to saying, that it is possible, that it is not unlikely, that the heart derives a power of motion from the nerves.[1]—These contradictions, with which several justly celebrated writers have reproached him, amongst others MM. Prochaska,[2] Behrends,[3] Ernest Platner,[4] &c., proceed evidently from his not being able to reconcile the results of experiments with the influence of the nervous power over the motions of the heart: and, in rejecting this influence, finding it impossible to explain the use of the nerves of the heart, and the effect of the passions on this organ. Here is the great difficulty in the controversy of which we speak. Those who, like Fontana, formally rejected all intervention of the nervous influence, have been forced to admit that the nerves, destined to convey to every other part life, feeling, and motion, have no known use in the heart.[5]

Such consequences evidently disclose the insufficiency of the theory of Haller, and several of his followers have acknowledged the necessity of some modification of it, and admit the nervous power to be one of the principles on which irritability depends.

[1] Element. Physiol. lib. iv. sect. 5, p. 493, et alibi passim.

[2] Opera Minora, Viennæ, 1800, tom. ii. p. 90.

[3] Vol. iii. p. 4, of the Collection of Ludwig, entitled *Scriptores Neurolog. Minores Selecti*, Lipsiæ, 1791—5. Four volumes, in 4to.

[4] Vol. ii. p. 266 of the same Collection.

[5] Mémoires sur les Parties Sensibl. et. Irritab, tom. iii. p. 234. See also Caldani, ib. p. 471, and Le Traité sur le Venin de la Vipère, tom. ii. p. 169—171.

They are thus enabled to assign a use to the nerves of the heart, and to explain the influence of the passions on this organ. But when they have attempted to explain why the interruption of all communication between the brain and the heart does not stop the motions of the latter, they have been obliged to abandon the generally received opinion, which regards the brain as the only centre and source of nervous power, and have admitted, without any *direct proofs*, that that power is generated throughout the whole extent of the nervous system, even in the smallest nerves, and that it can exist for a certain time in the nerves of any part, independently of the brain. Among the authors of this opinion, the learned Professor Prochaska is one of those who has given the best account of it.[1] But when he applies it to the motions of the heart, and attempts to explain why they are independent of the will, and yet influenced by the passions, his opinion appears undecided. He has recourse to the ganglions,[2] and hesitates what function to ascribe to them. Sometimes he considers them as knots, as ligatures, so tight as to intercept all communication between the heart and *Sensorium Commune*,[3] in the calm and peaceful state of the system, but not sufficient to prevent the sensorium re-acting more or less powerfully on the heart in the agitation of the passions.[4] Sometimes he seems to believe that the interception is complete and constant, and that it is by the nerves of the eighth pair that the passions affect the heart;[5] and he seems to adopt the opinion of Winslow,[6] renewed by Win-

[1] Commentatio de Functionibus Systematis Nervosi, published in the third fasciculus of the Annotationes Academ. of this writer, and reprinted at Vienna, in his Opera Minora, in 1800.

[2] See the Preface.

[3] Those parts of the brain on which sensation and voluntary power seem to depend, have been called the Sensorium Commune

[4] Opera Minora, tom. ii. p. 165.

[5] Ibid. p. 167.

[6] Exposit. Anatom. Traité des Nerfs, § 364.

terl,[1] Johnstone,[2] Unzer,[3] Lecat,[4] Peffinger,[5] &c., that the ganglions are so many small brains. He admits at the same time that the nerves of feeling are distinct from those of motion, so that the heart cannot contract except when the impression of the stimulant on its cavities is transmitted to the ganglions by the nerves of feeling, and reflected on its fibres by the nerves of motion.[6] But besides that this opinion, even by the author's confession, is only a conjecture, it supposes, on the one hand, that the circulation would continue after the destruction of the spinal marrow; and, on the other, that the heart would cease to beat at the moment when its communication with the ganglions and the plexuses[7] is interrupted. Now both these suppositions are contradicted by facts.

These fruitless attempts to modify the theory of irritability by the intervention of the nervous power, have only increased the zeal of some authors to maintain that theory in its original purity; and as the use of the nerves of the heart was among the most embarrassing objections to it, M. Sœmmerring, one of the most profound anatomists of Germany, and Behrends, one of his most distinguished scholars, maintained, in 1792, that the heart has no nerves, and that all those which appear to enter it are expended on the coats of its arteries, without the fibres of the heart receiving a single thread;[8] an opinion which, far from removing all the difficulties, only renders the influence of the passions on the motions of the heart more inexplicable. These two authors

[1] Nov. Inflam. Theoria, Viennæ, 1767, cap. 5. p. 154.

[2] Essay on the Use of the Ganglions, 1771.

[3] Unzer, quoted by Prochaska, Oper. Minor. tom. ii. p. 169.

[4] Traité de l'Existence de la Nature, et des Propriétés du Fluide Nerveux, p. 225. Berlin, 1765.

[5] De Structura Nervorum, Argentorati, 1782, sect. 1, § 34, inserted in the Collection of Ludwig, vol. i.

[6] Opera Minor. tom. ii. p. 169.

[7] See the Preface.

[8] Behrends Dissertatio, qua demonstratur Cor Nervis carere, Moguntiæ, 1792, inserted in the third volume of the Collection of Ludwig.

maintain that the nerves of the heart support and increase the irritability of its arteries; but the existence of irritability in the arteries is still doubtful, and, were it demonstrated, it would be very strange if irritability depended on the nervous influence in the arteries; and in the heart, the most irritable of all the organs, it were wholly independent of this influence.

Science, however, has cause to rejoice at the groundless doubts proposed by M. Behrends respecting the nerves of the heart, since they have induced the learned Scarpa to take part in the dispute, and have procured for us his excellent work on the nerves of the heart.[1] M. Scarpa proves in that work that the nerves of the heart are as numerous, and are distributed in the same way, as in other muscles. He admits, with M. Prochaska, that sensibility and irritability are essentially united, and that the nervous influence is generated throughout the whole extent of the nerves; but he does not admit that the ganglions are so many little brains.[2] He seems to believe that the nervous influence, such as it exists in all the nerves, is of itself sufficient for the exercise of the different functions, and that it only wants the stimulant which excites it to action: that the stimulant of the muscles of voluntary motion comes from the brain, and that in ordinary states the blood is the stimulant of the heart; but that in vivid emotion the brain also becomes a stimulant to this organ.[3]

According to this opinion the heart ought to beat in the same manner, and with the same force, after decapitation, after the destruction of the spinal marrow, and after it is removed from the body. M. Scarpa himself compares the beating of the heart in apoplexy to that which we observe when it no longer communicates either with the brain or spinal marrow.[4] But we shall see in the sequel that it is very different. We must not omit a very

[1] Tab. Neurolog. ad Illust. Hist. Anat. Cardiacorum Nervorum, &c.

[2] Ibid. § 30. Ticini, 1794.

[3] Ibid. § 22, 24, 25, 26, 27, 29.

[4] Ibid. § 25.

important remark of this author, and which it is surprising was not sooner made: it respects the insensibility of the heart when we irritate the spinal marrow and the nerves of the heart. M. Scarpa observes, that that insensibility of which so much has been said, and which has been regarded as a demonstrative proof that the motions of the heart do not depend on the nerves, proves only that the nerves of the heart are not of the same kind with those of the muscles of voluntary motion, and that the nervous power does not in them obey the same laws.[1] This reflection is without doubt very judicious, and it is by an error of experimental logic that we are surprised not to obtain the same effects from the irritation of two orders of nerves wholly different.

The work of M. Scarpa did not induce Dr. Sœmmerring[2] to change his opinion, nor prevent Bichat from denying that the nervous power has any share in the motions of the heart.[3] This last writer maintains the existence of an animal and organic life, distinct from each other, and of a nervous system for each of these lives. The system of the ganglions, which he regards in the same point of view with the authors above quoted, as small brains, belongs to the organic life; and the system of the brain, to the animal life.[4] To be consistent with himself, Bichat should have admitted, like M. Prochaska, that the heart, the centre of organic life, derives from the ganglions the principle of its motions; but he has not done so. It is chiefly the galvanic experiments which has brought him into this inconsistency, because he had attempted in vain to produce contractions in the heart by galvanising its nerves; experiments on which M. Sœmmerring and Behrends had also endeavoured to support their opinion.

[1] Tab. Neurolog. ad Illust. Hist. Anat. Cardiacorum Nervorum, &c. § 20.

[2] Th. Sœmmerring de Corporis Humani Fabrica, trajecti ad Mænum, 1796, tom. iii. p. 30, 43, 46, 50, et ibid. 1800, tom. v. p. 43.

[3] Bichat, Recherch. Phys. sur la Vie et la Mort. Paris, 1800, Part ii. Art. 11, § 1.

[4] Ibid. Part i. Art. 6, § 4. Ibid. Art. 1. § 2.

These experiments may always succeed, as one of us found in 1797,[1] and three years before was found by Mr. Fowler.[2]

Such is a short but faithful account of the principal systems, by means of which authors have, since the discovery of the circulation of the blood, to this day, attempted to explain the motions of the heart. On taking a general view of them, we remark, that in all those invented before Haller,[3] the nervous power is considered, in one way or other, as one of the conditions essential to the production of the motions of the heart; and it is always and only in the brain that they place the seat of it. The nerves of the heart, therefore, had a determined use in all these systems, and one could easily understand why the heart is subject to the empire of the passions; but it was impossible to explain why the circulation continues in animals without the head, and why, in experiments on animals, the interruption of all communication between the brain and the heart does not stop the motions of the latter. Since Haller, irritability has been the basis of all these systems. In regarding that property as essential to the muscular fibre, and independent of the nervous power, the circulation in animals without the head, and the different phenomena observed in the experiments alluded to, present nothing that is not easily understood; but the use of the nerves of the heart, and the influence of the passions on that organ, become inexplicable. The necessity of removing these difficulties has produced two parties among the supporters of irritability. The one, zealous favourers of the doctrine of pure irritability, called to their aid the most improbable hypotheses, and all their efforts have only served to prove how difficult it is to support the cause they espouse. The other confounded the nervous power with

[1] M. de Humboldt, Expériences sur l'Irritation de la Fibre Nerveuse et Musculaire, publiées en 1797, et traduites en Français de deux ans après, tom. i. chap. 9.

[2] Experiments on Animal Electricity, by Richard Fowler, 1794.

[3] Also in those of Ens, of Stœhelin, and others of whom we have spoken.

irritability, which they consider as one of the functions of that power; but they have been obliged to admit, either with respect to the seat, or the manner of existence, of the nervous power, conditions, which, by their own confession, are far from being demonstrated, respecting which they are not agreed, and which, in the application they make of them to the motions of the heart, either do not wholly remove the old difficulties, or create new ones.

One may easily see why so little progress has been made in this great and long-disputed question. If we examine all that has been said on the subject since the days of Haller, we shall find that both sides have constantly brought forward nearly the same facts, the same experiments, and the same reasonings. The only new experiments are the application of galvanism to stimulate the nerves of the heart; and they are only new in appearance, for, from the time of Haller, electricity has been employed with the same view.[1] It is evident that science had nothing to expect from our pursuing a path trodden for nearly sixty years by so many celebrated men. It was necessary to open new roads; it was necessary to find or invent new modes of interrogating nature. It was, above all, necessary to introduce into physiological experiments that precision and severe logic to which other branches of physical science have, in our days, owed so great progress. It is this which the author of the memoir before us has done.

It was not the original object of M. le Gallois to explore the cause of the motions of the heart. He had adopted the theory of Haller on this subject, when experiments undertaken with other views led him to the singular conclusion, that it was impossible for him to understand his own experiments, without determining whether the nervous power influences the motions of the heart; and if so, in what way it has this effect. To make his work better understood, we shall relate on what occasion, and

[1] See, amongst others, Mém. sur les Parties Sensib. et. Irritab., tom. iii. p. 214.

by what chain of facts and reasonings, he was led to engage in this inquiry.

A peculiar case of labour some years ago excited in him a wish to know how long a full-grown fœtus can live without breathing, after all communication between it and the mother has ceased. That question, curious in itself, and of the first importance in the practice of midwifery and medical jurisprudence, had hardly been touched upon by authors. M. le Gallois undertook to resolve it by direct experiments on animals; and that the solution might be generally applicable, and extend to as many cases as possible, he placed the fœtus of animals in various situations similar to those in which the human fœtus is occasionally placed, when it ceases to communicate with the mother. Among these there is one which occurs too often, namely, the fœtus having the neck drawn asunder in artificial delivery by the feet. The author wished to know what happens to the fœtus in this case, whether it perishes at the instant of the separation, and how death takes place. He found that the trunk retains its life, and that if hæmorrhagy be prevented by throwing a ligature round the vessels of the neck, it dies in the same time and with the same symptoms as if, without taking off the head, respiration had been interrupted; and what completely demonstrated to him that a decapitated animal is in fact suffocated, is, that we may at pleasure prolong its existence by inflating the lungs to supply the place of natural respiration.

M. le Gallois concluded from these facts, that decapitation proves fatal by destroying the motions of inspiration, and that consequently the power on which these motions depend is in the brain; but that that on which the life of the trunk depends is in the trunk itself. Endeavouring to ascertain the precise seat of each of these powers, he found that that on which the motions of inspiration depend resides in that part of the medulla oblongata, that is, the elongation of the brain, which forms the spinal marrow,[1] from which the eighth pair of nerves take their rise; and that on which the life of the trunk depends, in the spinal marrow. It is

[1] See the Preface.

not by all the spinal marrow that every part of the body is animated, but only by that portion from which it receives its nerves; so that in destroying any particular part of the spinal marrow, we only destroy life in those parts of the body which correspond to that part. Besides, if we interrupt the circulation in any particular part of the spinal marrow, life is weakened, and soon extinguished in all the parts which receives nerve from it. There are, therefore, two ways of destroying life in any part of an animal; the one destroying that part of the spinal marrow from which it receives its nerves, the other interrupting the circulation in it.

It hence results, that two conditions are necessary to preserve the life of any part of the body, viz. the integrity of the corresponding part of the spinal marrow, and the circulation of the blood, and consequently that we may preserve life in any part of an animal as long as we can preserve in it these two conditions. We may, for example, preserve the life of the anterior parts after that of the posterior parts is destroyed, by destroying the corresponding portion of the spinal marrow, or *vice versâ.*

In all destructions of the spinal marrow, even where the death is sudden, it is instantaneous only in the parts which receive their nerves from the destroyed part, and only extends to the rest of the body at the end of a certain time; but this time is fixed, and no means can prolong it. It is the same in animals of the same kind and of the same age; and the longer, the nearer the animal is to the time of its birth.

M. le Gallois, who had so often decapitated rabbits of different ages, had always remarked that the head, separated from the body, continued to gasp during a time determined by the age. This time was evidently the same as after destruction of the spinal marrow. Now it is evident that after decapitation there can be no circulation in the head, and that the gaspings that take place in that case can only continue for the time during which life may exist in the brain, after the total ceasing of the circulation. This was the first indication which M. le Gallois had, that when the partial

destruction of the spinal marrow occasions death throughout all the rest of the body, it is because it suddenly arrests the circulation; the truth of which position he ascertained by experiment.

He wished to ascertain in a more direct manner, if the circulation actually ceases at the moment the spinal marrow is destroyed. The absence of hæmorrhagy and the emptiness of the arteries were the most evident signs that he could have of the circulation having ceased; and he found, in fact, that soon after the above operation, the arteries which convey the blood to the head were found empty, and the amputation of the limbs occasioned no hæmorrhagy, though performed near to the trunk, and before life was extinct in the parts of which the spinal marrow had not been destroyed. In a word, all the signs which show the state of the circulation demonstrated to him, that when the destruction of any part of the spinal marrow suddenly occasions death in the rest of the body, it is by stopping this function; and this effect takes place, not because the motion of the heart immediately ceases, but because it is not capable of throwing the blood even into the carotids.

Hence it follows, that it is in the spinal marrow that the power on which the motion of the heart depends resides, and in the whole of it, since the destruction of any one of its three portions is capable of stopping the circulation. It also follows that each portion of the spinal marrow influences life in two different ways; by the one it is essential to the existence of life in the parts which receive nerves from it; by the other, it preserves it throughout the body in general, by contributing to furnish to the organs which receive nerves from the great sympathetic, and particularly to the heart, the life and power (*le principe de force et de vie*) necessary to the performance of their functions.

Thus we see, that to make the anterior or posterior parts of an animal living after killing the rest of the body, by destroying the corresponding parts of the spinal marrow, we must prevent the destruction of these parts from stopping the circulation. Now this is easily done by diminishing the sum of the forces which the

heart must impart for the support of the circulation, in proportion as we diminish the power which it receives from the spinal marrow. It is sufficient for this purpose to diminish by ligatures, thrown around the arteries, the extent of the parts to which the heart sends the blood. The destruction of the part of the spinal marrow which lies in the loins is quickly fatal to rabbits at or beyond the age of twenty days; but this is not the case if we previously throw a ligature round the aorta in the abdomen between the two great arteries which supply, the one the uppermost viscera of this cavity, and the other, the membrane by which the intestines are suspended.[1]

The application of this principle to other parts of the body leads to the singular conclusion, that in order to maintain life in rabbits of a certain age, after the destruction of the cervical part of the spinal marrow, we must previously cut off the head. They certainly die if this part of the spinal marrow is destroyed without previous decapitation. This fact ceases to surprise, when we reflect that by decapitation we lessen by the head the extent of the circulation, and that by that means the heart having need of less force to support the circulation, we may enfeeble it by the destruction of the cervical part of the spinal marrow, without destroying the circulation.

One may easily conceive that any other operation capable of suspending or considerably enfeebling the circulation in any part of an animal may produce a similar effect; and enable us, in like manner, to destroy such a portion of the spinal marrow, as would have been fatal without this previous operation. This is what happens in the partial destruction of the spinal marrow itself. It has two effects on the circulation: by the one it enfeebles it, generally by depriving the heart of that share of its power which it receives from the part of the spinal marrow that has been destroyed; by the other, without wholly destroying the circulation in the parts which are thus deprived of life, it in a great degree lessens it in a way in some measure similar to the effect of ligatures thrown

[1] See Preface.

round the arteries of these parts. But this effect is not remarked till a few minutes after the destruction of the spinal marrow. Thus it is the destruction of the first part of the spinal marrow which enables us to destroy a second; this a third, and so on. For example, when, by cutting off the head of a rabbit, we are enabled to destroy the cervical part of the spinal marrow, the destruction of that part in a certain number of minutes enables us to destroy the fourth part of the dorsal portion of the spinal marrow, and thus by continuing to destroy parts of similar extent, by intervals, we may at length destroy the whole of this portion of the spinal marrow without stopping the circulation, which is then supported by the lumbar portion only.

We may collect from what has just been said, that in rabbits, each portion of the spinal marrow bestows on the heart power sufficient to support the circulation in all those parts which correspond to that portion, and consequently, that in cutting a rabbit transversely, it would be possible to make each portion live for an indefinite time, if the lungs and the heart, necessary for the formation and circulation of arterial blood, could make part of it. But they can only make part of the chest, and one may very well maintain the life of the chest alone and insulated, after having cut off both the head and posterior parts, and prevented hæmorrhagy by proper ligatures, and that even in rabbits thirty days old or more.

Such are the principal results of M. le Gallois' researches—results which, arising one from the other, and mutually supporting each other, are founded on direct experiments, made with a precision hitherto unknown in physiology.

After giving an account of the repetition by M. le Gallois, in their presence, of the experiments on which his inferences are founded, the committee proceed:—

These experiments appear to us completely to confirm all the inferences which the author has deduced from them, and with which he finishes his memoir. To confine ourselves here to the principal points, we shall say, that we regard as demonstrated,—

1st. That the cause of all the motions of inspiration has its seat near that part of the medulla oblongata which gives rise to the nerves of the eighth pair.

2d. That the cause which animates each part of the body resides in the portion of the spinal marrow from which the nerves of that part are derived.

3d. That in like manner it is from the spinal marrow that the heart derives its life and its powers; but, from the whole spinal marrow, and not merely from any particular part of it.

4th. That the great sympathetic nerve takes its rise from the spinal marrow, and that the particular character of that nerve is to bring every part to which it is distributed under the immediate influence of the whole nervous power.

These results readily explain all the difficulties which have arisen since the days of Haller, respecting the causes of the motions of the heart. The reader will recollect that the principal of these are, 1st. Why does the heart receive nerves? 2d. Why is it influenced by the passions? 3d. Why is it not subjected to the will? 4th. Why does the circulation continue in decapitated animals and those born without the brain? He will recollect also, that till now no explanation has been able to reconcile these points, or at least has not been able to do so without the aid of hypotheses which we have seen give rise to other difficulties. But now we easily conceive why the heart receives nerves, and why it is so eminently subject to the influence of the passions, because it is animated by the whole of the spinal marrow. It does not obey the will, because none of the organs which are under the influence of the whole nervous power are subject to it. In fine, the circulation continues in decapitated animals and those without the brain, because the motions of the heart do not depend on the brain, or only depend upon it in a secondary way. We ought to remark, that this last point, on which M. le Gallois has thrown so much light, presents only confusion and errors in authors of the old school of Haller, as well as in those of the new school. None of them have distin-

guished the motions of the heart which take place after decapitation, from those which we observe after the excision of this organ, or after the destruction of the spinal marrow; and they have thought that both were equally capable of maintaining the circulation. But these motions differ essentially. The latter have no power to support the circulation; they are quite similar to the feeble movements which we may excite in the other muscles for some time after death. M. le Gallois calls them motions of irritability, without attaching for the present any other meaning to the term, but that of expressing certain phenomena after death.

We have still one task to perform; to point out what particularly belongs to M. le Gallois in the work which is the object of this report, and what others are entitled to claim.

We can affirm, without fear of contradiction, that everything in this work belongs to him. To be convinced of this, it is only necessary to read his memoir with attention. Chance suggested to him the idea of his first experiment, and that experiment led him to all the others, each of them being suggested to him, and, as one may say, forced upon him by that which preceded it. In following him step by step, one observes that his own method has been his only guide, and that it is that alone which has inspired him. Thus, it is a thing without example in Physiology, to see a work of such length, in which all the parts are so connected, so dependent on each other, that to have the complete explanation of any one fact, it is necessary to recur to all those by which the author arrived at it, and in which it is impossible to deny one inference without denying all those which precede, and disturbing all those which follow it.

One might have expected that in researches so numerous, and which, by the importance of the questions they embrace, have commanded the attention of a great number of philosophers, the author would often have been led, even in confining himself to his own method, to repeat experiments which had been made by others; yet among all the experiments found in his memoir we

have remarked only two which had been made before him; one by Fontana, the other by Stenon. The first[1] consists in inflating the lungs, and thus preserving the life of an animal after decapitation. Fontana only made that experiment to supply oxygen to the venous blood; and one may easily perceive that he was a stranger to the object before us. As the experiment was unconnected with any other subject, and did not serve as a proof of any point of doctrine, little attention was paid to it; and it was confounded with many other facts, showing that even warm-blooded animals may live after decapitation, without its being suspected that it was the decapitation which enabled them to live in that state. Hence it is that this experiment remained almost unknown except in some of the Schools of England and Germany; and M. le Gallois was wholly ignorant of it when he communicated to the Society of Medicine at Paris his first inquiries into the functions of the spinal marrow. Besides, this experiment in the hands of M. le Gallois was only one of the means by which he demonstrated two of his principal discoveries, namely, that the cause of the motions of inspiration has its seat in the medulla oblongata; and that the cause of life in the trunk resides in the spinal marrow.

The experiment of Stenon is that in which the aorta[2] is tied in the belly, and then untied, to show that the interruption of the circulation in any part occasions paralysis of that part, and that the return of the blood restores life to it. This experiment is well known, and has often been repeated. Some of the authors who have made it, had in view to prove that the contractions of the muscles depend on the action of the blood on their fibres; others, that the sensibility of every part depends on the circulation; and in both views it served equally to prove or disprove the point, according to the manner in which it was made. Thus, when they secured the aorta, the feeling and mo-

[1] Fontana, Traité sur le Venin de la Vipère, &c., tom. i. p. 317.

[2] See the Preface.

tion of the lower parts of the body quickly ceased.[1] But when the ligature was made lower, and only on one of the arteries into which this vessel divides in the loins, although in this case the circulation was wholly interrupted in the corresponding member, feeling and motion continued in it for a long time.[2] In these opposite results each author did not fail to adopt those which favoured his own opinion; and he thought himself authorised to do so, as the real cause of the difference was unknown.

In the hands of M. le Gallois the same experiment shows itself under a very different aspect, and assumes a determined meaning. It is evident that feeling and motion ceasing in the hinder parts, from a ligature being thrown round the aorta, arises from its being only in this case that the circulation is interrupted in that portion of the spinal marrow which gives rise to the nerves of these parts. Such are the only experiments of M. le Gallois, as far as we know, which can be claimed by others; but besides that the manner in which they make a part of his work renders them his own, it appears to us that the new points of view, under which he presents them, and the precision of the details and clearness of the results which he has substituted for the uncertainty and obscurity in which they were formerly involved, have made them experiments wholly new.

We shall finish by a few words on an opinion of M. Prochaska, which may be believed to be similar to that which M. le Gallois has demonstrated respecting the functions of the spinal marrow. That author places the sensorium commune in the brain and spinal marrow conjointly.[3] But it is necessary to be aware that he thinks that the nervous power is generated throughout the whole extent of the nervous system, so that every part

[1] Lorry, Journal de Méd. An. 1757, p. 15. Haller, Mém. sur le Mouvement du Sang, p. 203, Exp. 52.

[2] Schwenke, Hæmatol. p. 8. The 57th and 58th Experiments of Haller, p. 205, are of the same kind.

[3] Opera Minor., tom. ii. p. 51. Before him Marherr, Hartley, &c., had been of the same opinion.

derives from its own nerves, taken alone, the cause of its life and of its movements.[1] He only regards the sensorium as a central point, where the nerves of feeling, as well as those of motion, meet and communicate, and which establishes the connexion between the different parts of the body.[2] On the contrary, M. le Gallois has demonstrated that the spinal marrow is not merely a means of communication between different parts, but that from it the cause of the life and power of the whole body proceeds. And what proves that M. Prochaska, in announcing his opinion, which besides he only mentions as a thing probable,[3] was far from suspecting the true functions of the spinal marrow, is, that he regards it as only a great bundle of nerves, *crassus funis nerveus.*[4]

In a word, it appears to us that we may say of the authors who have had some views on the subjects of which M. le Gallois treats, what M. Laplace has said with so much justice on a similar occasion. One may there meet with some truths, but they are almost always mixed with so many errors, that their discovery belongs only to him who, separating them from this mixture, succeeds by calculation or observation in effectually establishing them.[5]

The opinion of your Committee is, that the work of M. le Gallois is one of the most excellent, and certainly the most important, which has appeared in physiology since the learned experiments of Haller; that this work will make an epoch in that science over which it must spread a new light; that its author, so modest, so laborious, so meritorious, deserves that the Class bestow on him its especial commendation, and all the encouragement which it can give. They cannot help adding, that the memoir of which they have given an account is worthy to occupy a distinguished place in the Transactions of learned cor-

[1] Opera Minor, tom. ii. p. 82. [2] Ib. p. 151.

[3] Ib. p. 153. [4] Ib. p. 84.

[5] Mem. sur l'Adhésion des Corps à Surface des Fluides, dans la Biblioth. Britan. tom. xxxiv. p. 33.

respondents, if the publicity of the important discoveries contained in it may be deferred to the time, perhaps distant, of the publication of those Transactions.

(Signed) De Humboldt.
Halle.
Percy.

The Class approve their Report, and adopt its conclusions.

It moreover decrees, that the Report shall be printed in the History of the Class, and that the Committee of the Class shall make arrangements with M. le Gallois for defraying the expenses which have been occasioned by his experiments, and enabling him to continue them.

Certified to be conformable to the original,

G. Cuvier,
Perpetual Secretary.

CHAPTER II.

Observations on the foregoing Report.

It will be necessary, before the author enters on the account of his own experiments, to make some observations on the foregoing Report. As a review of the state of our knowledge of the subject at the time M. le Gallois began his experiments, it appears to be accurate, well arranged, and sufficiently comprehensive. As an account of his experiments and opinions, nothing, as far as I can judge, can be more clear and correct; as an estimate of the merits of his work, it does not seem to me to deserve the same praise. It overlooks defects, both in the experiments and reasonings of M. le Gallois, of such moment as wholly to invalidate all his most important conclusions; and to leave him the discoverer of certain unconnected though very valuable facts, in-

stead of the author of a new system, founded, as the Report alleges, on a basis never to be shaken.

M. le Gallois has demonstrated, that the sudden destruction of any considerable portion of the spinal marrow so enfeebles the power of the heart, that it is no longer capable of supporting the circulation. He has also shown that the same portion of the spinal marrow, the sudden destruction of which destroys the circulation, may be destroyed by small parts without materially affecting it. The question then arises, if, as M. le Gallois supposes, the power of the heart is derived from the spinal marrow, and necessarily ceases when any considerable part of it can no longer perform its functions, why does the particular mode of destroying it make so great a difference in the result? This difficulty occasioned so much trouble to M. le Gallois, that it had nearly induced him to abandon the inquiry. "Après bien des efforts inutiles pour porter la lumière dans cette ténébreuse question, je pris le parti de l'abandonner, non sans regret d'y avoir sacrifié un grand nombre d'animaux, et perdu beaucoup de temps." Just before, he observes, "En un mot, j'eus presque autant de résultats différens que d'expériences." And indeed the apparent contradictions in the results of M. le Gallois' experiments are such, as at first view to have persuaded me that some of his experiments were inaccurate; on repeating many of them, however, I was convinced of their accuracy.

He attempts, we have seen, to explain the difficulty in the following manner. He has shown that if ligatures be thrown round the large vessels, at no great distance from the heart, so as greatly to lessen the extent of the circulation, this organ can still support it, notwithstanding the destruction of such a portion of the spinal marrow as would, under ordinary circumstances, have destroyed it. On the same principle accoucheurs apply tourniquets to the limbs in cases of profuse hæmorrhagy. Now, M. le Gallois supposes, that the power of the blood-vessels, as well as that of the heart, depending on the spinal marrow, we greatly impair the vigour of the circulation in any part by destroy-

ing that portion of the spinal marrow by which its nervous power is supplied; and, therefore, that when any portion of the spinal marrow is destroyed by small parts, the vigour of the circulation in the corresponding parts of the body being greatly impaired, nearly the same effect is produced as if ligatures had been thrown round their vessels. It might here be objected, that when a considerable portion of the spinal marrow is at once destroyed, the power of the vessels corresponding to this portion being lost, the effect produced by the ligatures should still be observed. To this I suppose M. le Gallois would have replied, that as it requires some time for the destruction of any part of the spinal marrow to produce its effect on the vessels, when a large portion is destroyed at once, the vessels not accommodating themselves to the rapid destruction of the successive parts of the spinal marrow, the circulation is lost.

The foregoing explanation resting wholly on the position, that the vessels of any part are debilitated when deprived of the influence of the corresponding part of the spinal marrow, it was incumbent on the committee to inquire by what experiments M. le Gallois had established it. This question, however, is overlooked by them; and, on reviewing the experiments of M. le Gallois, we find none from which any such inference can be drawn. He attempts to support it only by experiments not properly bearing on the point; although, if the position be correct, the simplest experiments are sufficient to establish it. It is impossible from his experiments to say, whether the diminished circulation in the parts in question arose directly from the destruction of part of the spinal marrow, or from the lessened power of the heart.

Another error, of even greater consequence in the reasonings of M. le Gallois, which is also overlooked by the committee, is his inference that the spinal marrow possesses an influence over the heart not possessed by the brain; because he found that removing the brain produces little or no effect on the action of the heart, while crushing the whole, or a considerable part of the spinal marrow, greatly enfeebles it. But to obtain this inference

it is evident that the brain and spinal marrow must be subjected to the same power. They ought both to have been removed, or both crushed.

The inferences which M. le Gallois makes from the effects of instantly killing the animal by crushing the spinal marrow, are in another respect incorrect. There are two ways in which we may account for the power of the heart, or of the blood-vessels, being destroyed by crushing the spinal marrow. Either the heart and blood-vessels derive their power from the spinal marrow, and consequently lose it on the destruction of the whole or a considerable part of that organ; or, deriving their power from some other source, they are influenced by agents acting on the spinal marrow. It was incumbent on M. le Gallois, therefore, to ascertain by experiment in which of these ways crushing the spinal marrow produces the effects he observed. But he does not even seem aware, that it may act in any other than the way he supposes.

By the same mode of reasoning, the inference which he draws from the restoration to life of the lower parts of an animal when a ligature, which has been thrown round the abdominal aorta, is removed, is inadmissible; namely, that in the experiment in which the circulation in every part is destroyed by crushing the spinal marrow, and we find that we cannot by any means restore it, this is to be ascribed to the absence of the influence of the spinal marrow. The same result may arise, it is evident, from the heart and blood-vessels, supposing them to derive their power from some other source, being so deranged by a powerful agent acting through the spinal marrow, that they are no longer capable of performing their functions. M. le Gallois relates no experiment to prove that his explanation ought to be admitted in preference to this; and the committee speak as if no inference, but that of M. le Gallois, could be drawn from the experiment.

Nor is M. le Gallois' inference respecting the origin of the great sympathetic nerve warranted by his experiments; namely, that it arises wholly from the spinal marrow. It is true that he

has found, that through this nerve a powerful agent, applied to any considerable portion of the spinal marrow, is capable of enfeebling the power of the heart; but nothing said by M. le Gallois proves that the heart may not also be affected in the same way through the brain.

A position on which much of the reasonings of M. le Gallois rests, which is admitted by the committee, but of which we find no proof in the experiments of this author, is, that the contractions of the heart, which recur after the sudden death caused by crushing the spinal marrow, are of a nature different from those which support the circulation. Observing that after the spinal marrow is thus destroyed, the contractions of the heart are too feeble to support the circulation, without farther inquiry he concludes, that these contractions do not merely differ in degree from those which support the circulation, but, existing independently of the spinal marrow, are wholly of a different nature.

The contractions of the heart, after it is removed from the body, are regarded by M. le Gallois as analogous to those which remain after the spinal marrow is crushed; and he regards in the same light the contractions which may be excited for a short time after death in the muscles of voluntary motion. Had M. le Gallois' mind been unbiassed by his peculiar views of the subject, he would have easily observed a striking difference between the action of the heart, immediately after death caused by suddenly crushing the spinal marrow, and its action immediately after it is removed from the body. In the former instance it is feeble and fluttering, gradually, especially in the cold-blooded animal, becoming stronger and more regular; in the latter instance, it is comparatively strong and regular, gradually, and in the cold-blooded animal very slowly, becoming more feeble. With respect to the contractions of the muscles of voluntary motion after death, it is generally known that these muscles may for some time be excited to the perfect performance of their function. They can be made to move the limbs precisely as they did before the death of the animal. But whether they move them as

forcibly or not, and whether or not the heart beats as forcibly after it is removed from the body, as while it supported the circulation, as far as we can see, the action of both is of the same nature as when they performed their usual functions ; and M. le Gallois has adduced no proof whatever of its being of a different nature. The experiment, indeed, in which he lessens the extent of the circulation by ligatures, and thus enables the heart to support it after such a portion of the spinal marrow is crushed, as would otherwise have destroyed it, is a sufficient refutation of his own opinion. It proves that the effect of crushing a large portion of the spinal marrow is merely that of enfeebling, not changing, the nature of the action of the heart.

Another position of M. le Gallois, admitted by the committee, but not warranted by his experiments is, that the power, on which all the motions of respiration depend, has its seat near that part of the medulla oblongata,[1] which gives rise to the eighth pair of nerves. On this subject I shall hereafter have occasion to make many observations; and shall only remark here, that respiration is the most complicated of all our functions, and that if any of the powers necessary to it are withdrawn, its most essential motions are as quickly destroyed as if all these powers had ceased. Now M. le Gallois made no experiments to ascertain whether it is by the destruction of one or all of these powers, that respiration is destroyed by destroying this part of the medulla oblongata.

The argument employed by the committee in favour of M. le Gallois' opinions from the existence of fœtuses without the brain, is wholly invalidated by the fact, that fœtuses have been born alive without either brain or spinal marrow; for instances of which M. le Gallois himself refers, in the two hundredth and fifty-first page of his Treatise, to the *His. de l'Acad. des Sciences*, *An.* 1711, *Obs. Anat.* 3, and *An.* 1712, *Obs. Anat.* 6, but without attempting to show how it is possible to reconcile his opinions with the existence of such cases. The reader will find, in the

[1] See the Preface.

fifth volume of the Medico-Chirurgical Transactions, the case of a full-grown fœtus, without either brain or spinal marrow, described by Mr. Lawrence. I have seen a similar case. In this case, as in that mentioned by Mr. Lawrence, in the place of the spinal marrow there only appeared a vascular membrane.

An inconsistency, of great importance in M. le Gallois' work, which he makes no attempt to explain, is overlooked by the committee. He observes, in the commencement of his work, "Ce que j'y ai dit du cœur pouvant s'appliquer aux autres organes des fonctions involontaires, la question peut être considérée plus généralement, comme la determination du siége du principe qui préside à cet ordre de fonctions."[1] Yet he shows that decapitation does not influence the function of the heart, while the division of the eighth pair of nerves injures that both of the lungs[2] and stomach.

It appears from what has been said, as far as I am capable of judging, that the experiments of M. le Gallois do not warrant any of the positions stated by the committee as the result of these experiments.[3]

If these results be not legitimate inferences from the experiments of M. le Gallois, the explanations of the long-contested

[1] Avant-propos, page 1.

[2] I speak here of the functions of the lungs themselves, not of the muscles of respiration.

[3] "1°. Que le principe de tous les mouvemens inspiratoires a son siége vers cet endroit de la moëlle allongée qui donne naissance aux nerfs de la huitième paire;

"2°. Que le principe qui anime chaque partie du corps réside dans ce lieu de la moëlle épinière duquel naissent les nerfs de cette partie;

"3°. Que c'est pareillement dans la moëlle épinière que le cœur puise le principe de sa vie et de ses forces; mais dans cette moëlle toute entière, et non pas seulement dans une portion circonscrite;

"4°. Que le grand sympathique prend naissance dans le moëlle épinière, et que le caractère particulier de ce nerf est de mettre chacune des parties, auxquelles il se distribue, sous l'influence immédiate de toute la puissance nerveuse;" that is, of the whole of the spinal marrow, which M. le Gallois regards as the seat of the nervous power.

points respecting the action of the heart, founded on them, are inadmissible; namely, that the heart is supplied with nerves, because it derives its power from the spinal marrow; that it is influenced by the passions, because the brain acts upon it through the spinal marrow; that it does not obey the will, because no organ influenced by every part of the nervous power, that is, of the spinal marrow, does obey the will; (it may here be remarked, that were this position admitted, it would by no means explain why the motions of the heart are independent of the will, though influenced by the passions;) and that the circulation continues in decapitated animals, and those born without the brain, because its direct dependence is not on the brain, but on the spinal marrow.

From all that has been said, it appears that we must dissent from the following opinion of the Committee; " Ces résultats resolvent sans peine toutes les difficultés qui se sont élevées depuis Haller sur les causes des mouvemens du cœur." The experiments of M. le Gallois, indeed, by ascertaining some facts of great importance, while others immediately connected with them escaped his observation, have left the subject in greater confusion than he found it. Instead of removing the difficulties which formerly existed, the valuable additions which he has made to our knowledge have shown us others.

The heart's being subject to the passions, yet independent of the influence of the brain, on which so much has been written, does not seem to imply a more direct contradiction, than that the destruction of the same part of the spinal marrow should, according to the way in which it is effected, either destroy the function of the heart, or little, if at all, influence it. I have had occasion to observe, that M. le Gallois' explanation of this apparent contradiction is not a legitimate inference from his experiments; and shall soon relate some so simple, that it is impossible to be deceived in their results, which directly refute it.

Why, if the power of the heart depends on the spinal marrow

as it appears to do from the experiments of M. le Gallois, have fœtuses been born alive where no spinal marrow had ever existed?

Why, if the power of the heart depends on the spinal marrow, does it continue to perform its usual motions after it is removed from the body?

Why, if (as M. le Gallois maintains, and it is generally admitted) the various organs of involuntary motion bear the same relation to the nervous system, is the function of the heart uninfluenced by decapitation, and that of the stomach and lungs immediately impaired by dividing or throwing a ligature round the eighth pair of nerves?

Why do the motions of respiration cease on the destruction of the medulla oblongata, since the nerves of the muscles employed in respiration arise from the spinal marrow, which M. le Gallois has proved to be capable of exciting the muscles independently of the brain? He considers this subject at length in the thirty-fifth and following pages of his treatise, and admits that he can give no explanation of it, calling it "one of the great mysteries of the nervous power, the discovery of which will throw the strongest light on the mechanism of the functions of that wonderful power.

All these apparent contradictions, it is evident, as well as those which existed previously to the discoveries of M. le Gallois, must be reconciled, before we can possess any correct views of the general laws of our frame. The doctrine which cannot reconcile them must be erroneous.

PART II.

EXPERIMENTS MADE WITH A VIEW TO ASCERTAIN THE GENERAL LAWS OF THE FUNCTIONS OF LIFE IN THE MORE PERFECT ANIMALS, AND THE PHYSIOLOGICAL INFERENCES TO WHICH THEY LEAD.

It appears from the most cursory review of the structure and functions of the animal body, that two great systems, the sanguiferous and nervous, pervade every part of it.

The sanguiferous system, it is evident, may be divided into three parts; the heart, the vessels of circulation, and the vessels of secretion and assimilation. In the following inquiry I shall, in the first place, endeavour to ascertain the principle on which the action of the heart and the vessels of circulation depends, and the relation which subsists between them and the nervous system. I shall then consider the principle on which the action of the muscles of voluntary motion depends, and the relation which they bear to this system. The comparative effects of stimulants acting through the brain and spinal marrow, on the heart and muscles of voluntary motion, will next be investigated. An account of the experiments on these branches of the subject, though not in the order in which they are here detailed, was presented to the Royal Society in two papers, composed while I was still engaged in this part of the inquiry, and published in the Philosophical Transactions of 1815.

The next object of inquiry will be the principle on which the action of the secreting and assimilating vessels depends, and the relation which they bear to the nervous system.

The principle on which the action of the alimentary canal depends will then be considered, and its relation to this system.

These subjects will lead to experiments and observations on the use of the ganglions, and the cause of animal temperature.

The relation which the different functions of the animal body bear to each other, and the order in which they cease in dying, will form the next subject of investigation; and lastly I shall endeavour, by various experiments, to determine how far we are enabled to advance in ascertaining the nature of the vital powers; and this Part of the Treatise will conclude with a review of the inferences obtained from the various experiments and observations which shall have been laid before the reader.

CHAPTER I.

On the Principle on which the Action of the Heart and Vessels of Circulation depends.

As it is now generally admitted by physiologists, as appears from the report just laid before the reader, that the heart is capable of performing its functions after the brain is removed, the first question which presents itself is, How far does the power of this organ depend on the influence of the spinal marrow, from which, we have seen, M. le Gallois maintains that it is wholly derived?

Exp. 1. A rabbit was killed by a blow on the occiput, the usual mode of killing this animal for the table. The author uses the words death and kill in the usual acceptation, not implying the ceasing of all the functions. The final ceasing of the sensitive functions alone, as will afterwards appear, is what we call death. After this explanation, no ambiguity can arise from the use of these terms. When the rabbit is killed in this way, the respiration immediately ceases; but the action of the heart and the circulation continue, and may be supported for a considerable length of time by artificial

respiration, that is, by alternately throwing air into the lungs and allowing it to escape, as practised by Fontana, and since by Chirac, M. le Gallois, Sir Benjamin Brodie, and others.[1] This mode of killing the animal does not influence the result of the experiment, and has the double advantage of preventing the animal's sufferings, and his motions. Its greatest inconvenience is, that if care be not taken, considerable vessels are sometimes ruptured, and there is almost always some rupture of vessels, which of course tends to impair the vigour of the circulation. It is the usual mode of killing rabbits, and is that which was adopted in all the experiments on the newly-dead animal, except where the contrary is mentioned.

As soon as the animal was killed, the spinal marrow was laid bare from the head to the beginning of the dorsal vertebræ, the circulation being supported by artificial respiration. The chest was then opened, and the heart found beating regularly, and with considerable force. The spinal marrow, as far as it had been laid bare, was now wholly removed, but without in the least degree affecting the action of the heart. After this, the artificial respiration being frequently discontinued for a short time, we repeatedly saw the action of the heart become languid, and increase on renewing it. The skull was then opened, and the whole of the brain removed, but without any abatement of the action of the heart, which still continued to be more or less powerful, according as we discontinued or renewed artificial respiration. This being for a considerable time discontinued, the

[1] It appears from the first volume of the Philosophical Transactions, that Mr. Hook, in the year 1667, showed, in the presence of the Members of the Royal Society, not only that the life of a dog could be preserved for an hour after the thorax had been opened, and a great part of the diaphragm removed, by alternately inflating the lungs and allowing them to collapse so as to imitate respiration; but that the effect is nearly the same if the lungs are preserved in a state of permanent distension by air constantly thrown into them, and allowed to escape by small perforations made in their surface.

ventricles of the heart ceased to beat about half an hour after the removal of the brain. On renewing the respiration, however, the action of the ventricles was restored. The respiration was again discontinued and renewed, with the same effects.

Exp. 2. A rabbit was made insensible by applying opium to the brain. The spine was then opened between the cervical and dorsal vertebræ, the thorax laid open, and the action of the heart supported by artificial respiration. The force with which it beat was carefully observed, and the spinal marrow destroyed by running a *very small* wire up and down the spine, through the opening made in it, by which the action of the heart was not at all affected.

Exp. 3. In the foregoing experiments, it may be said, there was no direct proof of the continuance of the circulation after the spinal marrow was destroyed or removed. On this account, this and several of the following experiments were made. A rabbit was killed, and the circulation supported by artificial breathing. The large arteries which convey the blood to the head being exposed, were seen beating, a proof that the circulation continued.[1] The cervical part of the spinal marrow was then destroyed by a small hot wire, after which these arteries were still found beating.

Exp. 4. In a rabbit killed in the usual way, the whole spinal marrow was destroyed by a small hot wire, and the breathing artificially supported. One of the arteries of the neck was then laid bare. Its beating was evident, and on dividing it, florid blood flowed from it freely.

Exp. 5. The only difference between this and the last experiment was, that artificial breathing was not performed. In both, the spinal marrow was destroyed, by introducing a small hot wire through an opening between the cervical and dorsal vertebræ, first through the upper portion to the brain, then through the under portion to the end of the spine. On laying open one

[1] Towards the end of the Treatise, the author will have occasion to make some observations on the nature of this beating.

side of the neck, the artery was found beating. On dividing it, blood of a much darker colour than in the former experiment was thrown copiously from it *per saltum.*

Exp. 6. A rabbit was killed in the same way, and artificial respiration maintained. The spinal marrow from the base of the skull to the beginning of the dorsal vertebræ was removed, and the remaining part of it destroyed by a small hot wire. The same artery was then found beating, and, on dividing it, florid blood rushed out with great force *per saltum.*

Exp. 7. This experiment resembled the last, except that the spinal marrow, instead of being partly removed, was wholly destroyed by a hot wire, and artificial breathing was not performed previous to opening the artery, from which dark-coloured blood flowed *per saltum.* The lungs were then inflated, and florid blood soon began to flow copiously from the vessel, and appeared like a red stream mixing with the dark-coloured blood which had previously come from it. This experiment was repeated in the same manner, and with the same result, proving that the circulation was vigorous throughout the whole system, after the spinal marrow had been destroyed.

Exp. 8. In this experiment the rabbit was rendered insensible by a blow on the back part of the head, except that it still breathed. The spine was opened, and the spinal marrow destroyed, as in the preceding experiment. On introducing the wire through the spine into the brain, the breathing immediately ceased. The artery of the thigh was laid bare about two or three minutes after respiration had ceased. The beating of the artery was evident. On opening it, a dark-coloured blood flowed from it freely. Artificial respiration was now employed. In about half a minute, the blood, which continued to flow copiously from the artery, became of a highly florid colour. The other femoral artery was then opened, from which florid blood also flowed freely. When about an ounce of blood had flowed from the two vessels, the inflation of the lungs was discontinued, and the blood again flowed of a dark colour. On renewing the inflation of the lungs,

in less than half a minute it became of a florid colour. It continued to flow from the arteries altogether for seven minutes. Three minutes after the blood had ceased to flow from them, artificial respiration being continued, one of the great arteries of the neck was opened, from which a florid blood flowed in a free stream, to the amount of a drachm and a half. The flow from the artery ceased in eleven minutes after the first artery had been opened. Most of the blood was now of course evacuated. A good deal had been lost in opening the spine, which always happens. The left cavities of the heart were found nearly empty. The blood which remained in them was florid. The right cavities were full of dark blood.

Exp. 9. From various trials, we found that in such experiments the circulation ceases quite as soon without, as with the destruction of the spinal marrow. Loss of blood seems to be the chief cause which destroys it. If the living animal were operated upon, pain would also contribute to this effect. After the skull and spine were laid open, it was frequently found that the circulation was lost before either the brain or the spinal marrow had been disturbed. The circulation is particularly apt to fail, if artificial respiration is not carefully performed after the animal, being no longer capable of any sensation, ceases to breathe. In making such experiments, after opening the bone, it is always necessary to ascertain whether the circulation continues, before we destroy or remove the brain or spinal marrow. As little blood is lost in this part of the operation, when the arteries are beating before, we always find them beating after it. The result of this experiment is still more striking in cold-blooded animals, in which death takes place so slowly, that the circulation continues long after the destruction of the brain and spinal marrow.

Exp. 10. The brain and spinal marrow of a frog were removed at the same time. On opening the thorax, the heart was found performing the circulation freely.

It appears from these experiments that the action of the heart is as independent of every part of the spinal marrow as of the

brain; and, consequently, that the opinion of M. le Gallois, that it derives its power from that organ, and particularly from the cervical part of it, must be regarded as erroneous. I shall soon have occasion to consider the facts which led M. le Gallois to this opinion; the cause of which, it will appear, is very different from that to which they are referred by M. le Gallois. I am now to inquire, whether the action of the vessels of circulation is also independent of the brain and spinal marrow.

The following experiments, and some others which I shall have occasion to relate, were made on the capillaries of the frog, which, from the extent and transparency of the web of its hind feet, and from the great tenacity of life possessed by the frog, appeared the best subject for such experiments. It has been questioned, how far inferences drawn from experiments made on cold-blooded animals can be supposed to apply to those of warm blood. Both Fontana and Dr. Monro observe, that in their experiments they found the systems of both obeying the same laws. The experiments I have had occasion to make on both sets of animals tend to confirm this observation. There are certain circumstances in which they evidently differ, in all others they seem to agree. As there is no part of the warm-blooded animal on which such experiments on the vessels of circulation, as I shall have occasion to relate here and in the next chapter, can be made except the mesentery, many of them would be attended with much greater suffering in this, than in the cold-blooded animal; and some of them, from the warm-blooded animal being less tenacious of life, could not be so satisfactorily performed on it.

Exp. 11. A frog was killed suddenly by cutting off the head, after a ligature had been applied round the neck to prevent loss of blood; much loss of blood immediately destroys the circulation in the extremities. The spinal marrow was then destroyed by a small wire. On bringing the web of one of the hind legs before the microscope, I found the circulation in it vigorous for a few minutes, and in all respects resembling that in the

web of a healthy frog. This experiment was repeated with the same result.

Exp. 12. A frog was immediately killed by destroying the brain and spinal marrow by a small wire. After it had lain dead for several minutes, part of the web of one of the hind legs being brought before the microscope, the blood was seen circulating in it as rapidly as in the web of a healthy frog. In making such experiments it is necessary to be aware, that handling and stretching the web tends to impair the vigour of the circulation in it. If this experiment is objected to on account of its being made on an animal of cold blood, I might, as far as the larger vessels are concerned, refer to several experiments just related, in which the circulation in the arteries of the neck and thigh of rabbits was free after the animal had been killed by a blow on the head, and the spinal marrow had been removed or destroyed; and with respect to the capillaries, in which the colour of the whole mass of blood was darkened or brightened by artificial respiration after both brain and spinal marrow had been destroyed.

It appears from these experiments that the vessels of circulation, even to their minutest extremities, like the heart, retain their power after the brain and spinal marrow are destroyed or removed; for it will hardly be asserted, that in these instances the power of the heart supports the motion of the blood in the vessels. Should this opinion be maintained, the reader will find it refuted, respecting animals of cold blood, by experiments related in the next chapter, and respecting animals of warm blood, by those related in Chapter X.

From the whole of the foregoing experiments it must be inferred, that the position of M. le Gallois, that the circulation in every part of the body depends on the corresponding part of the spinal marrow, and consequently his explanation of the destruction of the same portion of the spinal marrow destroying the circulation if suddenly effected, but failing to do so if effected slowly, is erroneous. [1]

[1] See *ante*.

It appears a necessary inference from the experiments related in this chapter, that the action of the heart and vessels of circulation depends on a power inherent in themselves, and having no direct dependence on the nervous system. Yet many facts, laid before the reader in the first part of this Inquiry, prove that a certain relation subsists between the nervous and sanguiferous systems. What this relation is, we are now to inquire.

CHAPTER II.

On the Relation which subsists between the Heart and Vessels of Circulation and the Nervous System.

It is generally supposed, we have seen, that the action of the heart cannot be influenced by stimulants applied to the brain and spinal marrow; and it seems at first sight almost a contradiction to suppose that it should be so, when we see that it cannot be influenced by the total removal of these organs. Many reasons, however, induced me to try the effect on the heart, of stimulants so applied to the brain and spinal marrow, as not to excite any of the muscles of voluntary motion, whose action, both by throwing more blood towards the heart, and by agitating the animal, prevents our judging of their effect.

Exp. 13. A rabbit was killed in the usual way, the action of the heart supported by artificial respiration, and the brain and cervical part of the spinal marrow laid bare. The thorax was now opened, and the action of the heart, which beat with strength and regularity, observed. Spirit of wine was then applied to the spinal marrow, and a greatly increased action of the heart was the consequence. It was afterwards applied to the brain with the same effect. The increase of motion was immediate and decided in both cases. We could not perceive that it was greater in the one case than in the other.

Exp. 14. The foregoing experiment was repeated, with the difference, that the whole of the spinal marrow was laid bare. The motion of the heart was nearly, if not quite, as much influenced by the application of the stimulant to the dorsal, as to the cervical portion of the spinal marrow; but although evidently influenced, it was comparatively little influenced by its application to the lumbar portion.

Exp. 15. In this experiment, the rabbit was prepared in the same way as in the preceding experiments, except that that part of the brain alone which occupies the anterior part of the head was laid bare. The spirit of wine applied to this part of the brain produced as decided an effect on the motion of the heart as in those experiments. The spirit of wine was washed off, and a watery solution, first of opium, then of tobacco, applied, with the effect of an increase, but a much less increase of the heart's action than arose from the spirit of wine. The increased action was greater from the opium than from the tobacco. The first effect of both was soon succeeded by a more languid action of the heart than that which preceded their application to the brain. This effect was greatest, and came on soonest, when the tobacco was used; and an evident increase in the action of the heart always occurred when the tobacco was washed off. This could also be perceived, though in a less degree, when the opium was washed off.—Little or none of this debilitating effect was observed when the spirit of wine was washed off. After its stimulating effect had subsided, the action of the heart only returned to about the same degree as before the application of the stimulant.

Exp. 16. The foregoing experiment was repeated on an animal of cold blood.

Dr. Hastings had found, that immersing the hind legs of a frog in tincture of opium, in less than a minute deprives it of sensibility. This does not arise from any action of the opium; a watery solution of opium, we found, however strong, does not produce the effect. It is produced by simple spirit of wine. It

is remarkable, that if simple spirit of wine is used, the animal expresses pain; if tincture of opium, little or none. The author has just mentioned the reason why it is necessary, in order to judge of the result of this experiment, that the animal should be rendered insensible. A frog, being thus deprived of sensibility, the brain and spinal marrow were laid bare, and the chest opened. The heart was found contracting with vigour. Spirit of wine was applied to the spinal marrow with an immediate and evident increase of the action of the heart. It was then applied to the brain with the same effect. Watery solutions of opium and tobacco were also applied to both, with precisely the same effects as in the rabbit. The increase of action from the opium and tobacco was much less than from the spirit of wine, and was soon followed by a great diminution of action. The increase of action was least, and the diminution greatest from tobacco. On washing off the opium and tobacco with a wet sponge, the heart immediately beat more strongly. The different parts of this experiment were frequently repeated with the same result. It is remarkable that the motion of the heart could be affected by stimulants applied to the brain and spinal marrow, after they had all ceased to produce any effect on the muscles of voluntary motion through the medium of the nervous system, and long after the circulation had ceased.

Exp. 17. This experiment only differed from the last, in the cervical part of the spinal marrow and lower part of the brain being removed, and the stimulants applied only to that part of the brain which lies between the eyes of the frog. Spirit of wine, opium, and tobacco, thus applied, affected the motion of the heart quite as much, and precisely in the same way, as when they were applied to the entire brain and spinal marrow; a proof that the influence of the agents applied to the brain is not communicated to the heart as to the muscles of voluntary motion through the spinal marrow. When opium and tobacco were applied to the lower part of the spinal marrow, the motion of the heart

appeared to be hardly at all affected by them. It was evidently increased when spirit of wine was applied to the same part.

Exp. 18. It was found from repeated trials on the newly dead animal, that considerable pressure either of the brain or spinal marrow produces little or no effect on the action of the heart.

Thus it appears from the preceding experiments, that although the power of the heart is independent of the brain and spinal marrow, it is capable of being influenced through these organs both by stimulants and sedatives.

All that has been said of the power of the heart is strikingly illustrated by the following experiments.

Exp. 19. The reader has seen that if the brain and spinal marrow of a frog be removed, the heart continues to perform its function perfectly for many hours, nor does it seem at all immediately affected by their removal. But we find the effect very different when the most sudden and powerful agent is applied to them. If either the brain or spinal marrow be instantly crushed, the heart immediately feels it. The brain of a large frog was crushed by the blow of a hammer. The heart immediately performed a few quick and weak contractions. It then lay quite still for about half a minute. After this its beating returned, but it supported the circulation very imperfectly. In ten minutes its vigour was so far restored, that it again supported the circulation with freedom, but with less force than before the destruction of the brain. An instrument was then introduced between the heart and spine, and after ascertaining that this had produced no change in the action of the former, the spinal marrow was crushed by one blow, as the brain had been. The heart again beat quickly and feebly for a few seconds, and then remained still, and seemed wholly to have lost its power. In about half a minute it again began to beat, and in a few minutes acquired considerable power, and again supported the circulation. It beat more feebly, however, than before the spinal marrow was destroyed. It ceased

to beat in about an hour and a half after the brain had been destroyed. In another frog, after the brain and spinal marrow had been wholly removed without any further injury being done to them, the heart beat for nine hours, gradually becoming more languid.

From this experiment it appears that the heart not only retains its power long after the brain and spinal marrow are removed, but that if they are destroyed in such a way as through them to impair and almost destroy the action of the heart, it can recover the power of performing its function, after they no longer exist; precisely as a muscle of voluntary motion will by rest recover its excitability, although all its nerves have been divided.

Exp. 20. The foregoing experiment cannot be performed in the same way on warm-blooded animals, but it may be performed in a way equally satisfactory. Two rabbits were instantly killed by crushing the brain by a blow with a hard substance. In both, the heart immediately beat with an extremely feeble and fluttering motion.

Exp. 21. A rabbit was instantly killed by crushing, in the same way, only the anterior part of the brain. The effect on the heart was the same as in the preceding experiment.

Exp. 22. A strong ligature was thrown round the neck of a rabbit, and at the moment it was tightened, the head was cut off. The bleeding, except from the vessels defended by the bone, was restrained by the ligature. General spasms made the body hard for the space of between one and two minutes, so that the beating of the heart could not be felt. At the end of this time, the heart was felt through the side, both by Dr. Hastings and myself, beating regularly, and not more quickly than in health. The rabbits used in the three last experiments were of the same age.

Exp. 23. The following experiments are still more conclusive. A rabbit was instantly killed by crushing the anterior part of the brain by a blow, as in Experiment 21. The side was rendered hard by spasm for about half a minute. Neither during

this, nor after it, could the author perceive any motion of the heart by applying the hand to the side. The head was then cut off, about three quarters of a minute after the brain had been crushed. No blood spouted out, and very little ran from the vessels.

Exp. 24. A strong ligature was passed round the neck of a rabbit of the same age with that used in the preceding experiment. It was suddenly tightened, and the head cut off. In this instance little spasm took place, and the heart was found beating regularly under the finger for about three quarters of a minute. At the end of this time the ligature was slackened, and the blood spouted out to the distance of three feet, and continued to spout out with great force, till nearly the whole blood was evacuated.

Exp. 25. From the strength of the spine of a rabbit, and the situation of the neighbouring parts, it is impossible to crush it, without directly influencing the state of the heart by the blow. It was opened between the cervical and dorsal vertebræ, and the animal suddenly killedby a steel rod, of considerable thickness, passed through the cervical part. As in the experiments of M. le Gallois, the action of the heart was immediately debilitated. In the preceding experiments, the reader has seen both the cervical and other portions of the spinal marrow repeatedly slowly destroyed, or removed entirely, without at all influencing the action of the heart.

These experiments point out an easy solution of the difficulties stated by M. le Gallois in the 119th and following pages of his treatise. When in his experiments the greater part of the spinal marrow was destroyed by small portions at a time, comparatively little effect was produced on the heart; but when a considerable part of it was crushed at once, the power of the heart was so impaired, that the circulation ceased. Thus in some of his other experiments, where the injury was inflicted slowly, and where it was inflicted suddenly, the result was found to be different. He observes, that if the spinal marrow be divided near the back of

the head, and a certain part of it immediately destroyed, the circulation ceases. If some time intervene between the division and the destruction of precisely the same part, the circulation is not interrupted.

In his experiments, the spinal marrow was always crushed by a stilet, of the same dimensions with the cavity of the spine. In the corresponding experiments related in this treatise, the spinal marrow was either removed, or destroyed by a comparatively small wire, moved about in it till its functions ceased. The reader will easily understand, from the experiments just laid before him, why this apparently slight circumstance, which appeared to M. le Gallois so unaccountable that it nearly induced him to abandon the inquiry; and which, he explains in a way so inconsistent with the facts which have been laid before the reader, occasions so essential a difference in the result. The effect on the heart is the same whether the animal be previously killed by means that do not immediately affect this organ, or death be occasioned by the means which cause the instantaneous destruction of the brain or spinal marrow; that is, the effect on the heart is the same, whether the powers of the sensitive organs of the brain have been previously destroyed or not, so distinct are its sensitive and vital organs.

M. le Gallois, the reader has seen, maintains that affections of the brain influence the heart only through the medium of the spinal marrow. But in experiment 17th, after the lower part of the brain and cervical part of the spinal marrow of a frog had been removed, agents applied only to the anterior part of the brain, affected the action of the heart as much as when applied to the brain, while both this organ and the spinal marrow were entire. To remove any objection which may arise from the subject of this experiment having been an animal of cold blood, the following was made:—

Exp. 26. A rabbit was suddenly killed by dividing the spine near the head. The spine was then divided near the lower end also, and by means of a wire, introduced at these parts, the

spinal marrow was destroyed. Spirit of wine was then applied to the brain, which influenced the action of the heart as readily, and to as great a degree, as it does when the spinal marrow and all its connexions are entire.

We are now to inquire how far the vessels of circulation are capable of being influenced through the brain and spinal marrow.

In order to ascertain whether the vessels can be stimulated through these organs independently of their action on the heart, it is necessary, in the first place, to determine how far the vessels can support the motion of the blood independently of the heart.

M. Bichat[1] has shown that in a frog the motion of the blood continues in the capillaries after the heart no longer propels it; and this observation, we shall afterwards find, applies to the warm as well as the cold-blooded animal.

Exp. 27. A frog was decapitated without much loss of blood, and a ligature thrown round all the vessels attached to the heart; on the web of one of the hind legs being brought before the microscope, the circulation in it was found to be vigorous, and continued so for many minutes; at length gradually becoming more languid.

In endeavouring to proceed farther, the author found much difficulty. It is necessary, in order to ascertain the effect of stimulants applied to the brain or spinal marrow on the vessels of the web, to destroy the sensibility, to remove the heart, or throw a ligature around the great artery proceeding from it, and to lay open the cranium or spine. Were not the sensibility destroyed, the voluntary motions of the animal, which would continually occur, could not fail to accelerate the motion of the blood in the web.

Exp. 28. A frog was deprived of sensibility and voluntary power, by the upper parts of the body being immersed in lauda-

[1] Recherches Phys. sur la Vie et la Mort.

num; part of the cranium was then removed, after a ligature had been thrown round the neck to prevent loss of blood. The thorax was now opened, and the vessels attached to the heart included in a ligature, and either the cranium or spine laid open. But, notwithstanding this experiment was repeatedly performed with the greatest care, the circulation by all these preparatory means was so enfeebled, that although the blood still moved in the web, it was in so irregular and uncertain a way, that I never could arrive at any positive conclusion respecting the effect of the stimulant applied to the brain. After several fruitless attempts, therefore, I abandoned this mode of making the experiment.

Although the action both of the heart and muscles of voluntary motion so influence the effect of stimulants, applied to the brain, on the circulation in the foot, that, without wholly preventing it, no conclusion can be drawn, it is evident that the sedative lessening the power of the heart will not affect the result of the experiment, if it be made on the web of the frog. We have just seen, that the total ceasing of the action of the heart does not, for a considerable time, affect the circulation in it. The following experiments are decisive respecting the effect of the sedative, and of the stimulant, as far as this can be decisive, the action of the heart remaining. It is evident that the action of either stimulant or sedative is equally conclusive respecting the direct influence of the nervous system on the blood-vessels.

Exp. 29. Part of the cranium of a frog was removed, the web of one of the hind legs brought before the microscope, and the circulation in it observed. The animal was then rendered insensible by the immersion of the other hind leg in laudanum. The insensibility did not in the least affect the circulation in the web before the microscope. Spirit of wine was then applied to the brain, with an evident increase of the velocity of the blood in the web. The same effect was produced in a less degree by watery solutions of opium and tobacco. After the tobacco had been applied for about half a minute, the motion of the blood

was much less rapid than before its application. On washing off the tobacco the velocity of the blood increased, and was again lessened on applying it. This was repeated several times with the same effects. The following way of performing the experiment is equally conclusive.

Exp. 30. A frog was rendered nearly insensible by having its back immersed in laudanum. A ligature was then thrown round the neck to prevent loss of blood, part of the cranium removed, the web of one of the hind legs brought before the microscope, and the circulation in it, which was rapid, observed. A strong infusion of tobacco was then applied to the brain, with the effect of at first rendering the circulation more rapid. In about half a minute it became more languid, and soon stopped altogether. On the infusion of tobacco being washed off, the circulation returned and regained considerable vigour. The tobacco was several times applied to the brain and washed off, with the same effects. It may be observed, that when the circulation in the web had almost ceased after the tobacco had been washed off, its velocity was immediately increased on applying spirit of wine to the brain.

Thus it appears that the power of the blood-vessels, like that of the heart, is capable of being directly destroyed through the medium of the nervous system.

Exp. 31. The web of one of the hind legs of a frog was brought before the microscope, and while Dr. Hastings observed the circulation, which was vigorous, the brain was crushed by the blow of a hammer. The vessels of the web instantly lost their power, the circulation ceasing; an effect which cannot arise, we have seen, from the ceasing of the action of the heart. In a short time the blood again began to move, but with less force. This experiment was repeated with the same result. If the brain is not completely crushed, although the animal is killed, the blow, instead of destroying the circulation, increases its rapidity.

Exp. 32. The spine of a frog was laid open at the lower end, and the animal suddenly killed by a wire of nearly the same di-

mensions with its cavity, passed through it, as in M. le Gallois' experiments. The web of one of the hind legs was then brought before the microscope, and the circulation in it was found to have wholly ceased. In another frog, as the reader has seen, [1] the spinal marrow was destroyed by introducing in the same way, and moving in various directions, a wire much smaller than the cavity of the spine, yet the circulation in the web continued vigorous.

Exp. 33. Analogous to what happens with respect to the heart, as I shall have occasion to point out more particularly, I could never, either by chemical or mechanical agents, if we except crushing the brain or spinal marrow, excite any irregular action in the blood-vessels. Their action was only rendered more or less powerful.

The irregular appearances in the circulation in the web of a frog's foot, mentioned by Dr. Thomson, lately Professor of Military Surgery in the University of Edinburgh, in his Lectures on Inflammation, published a few years ago, and which he ascribes to inflammation, may be observed in any case, if the vessels be at all compressed in applying the foot to the microscope; and although they are not compressed, these appearances very generally occur when the circulation begins to fail. The blood will then stop and go on at intervals, and move backwards and forwards in the same vessel. The author has often, with the assistance of the microscope, watched the capillaries from the commencement of inflammation to its greatest height, when the part is about wholly to lose its vital power, in the mesentery of a rabbit, the web of a frog's foot, and the fins of fishes, without perceiving the least tendency to this irregular motion when the part viewed was so applied to the microscope as not to compress any of its vessels. [2] When

[1] See Experiment 12.

[2] An account of these experiments is published in the introduction to the author's Treatise on Symptomatic Fevers, and a plate given representing the state of the vessels in the different stages of inflammation. See also the last part of this Inquiry.

the circulation fails without any morbid distension of the vessels, the motion of the blood becomes irregular before it stops altogether; when it fails from morbid distension of the capillary vessels, which gives rise to the phenomena of inflammation, this irregularity is not perceived; the motion of the blood gradually becomes slower, till it ceases altogether.[1]

What are the simple results of the experiments related in this and the preceding chapter? The first set prove, that the power of the heart and vessels of circulation is independent of the brain and spinal marrow, for we find that the functions of the former organs continue after the latter are destroyed or removed, and that their removal is not attended with any immediate effect on the motions of the heart and vessels. The second set prove, that the action of the heart and vessels of circulation may be influenced by agents applied either to the brain or spinal marrow. It is as readily influenced by agents applied to the anterior part of the brain, as by those applied to the cervical part of the spinal marrow; and there is no part of either organ through which both may not be influenced. All this is what we should expect when we trace the origins of their nerves. To how different a view of the animal economy, compared with that now generally received in this country do, such facts lead, will appear from the concluding chapter of the present part of this Inquiry.

If it be said that the results of these experiments imply a contradiction, that we cannot suppose the power of the heart and vessels to be wholly independent of the brain and spinal marrow, and yet influenced by agents applied to them, the reply is, that such are the facts, of the truth of which any one may easily satisfy himself by experiments on the newly-dead animal.

On a closer examination of the phenomena of the nervous system, we shall find other similar difficulties. The experiments of M. le Gallois prove, in the most satisfactory manner, that a principal function of the spinal marrow is to excite the muscles

[1] This part of the subject will be resumed in Part III.

of voluntary motion, and that it can perform this office independently of the brain. It performs it after the brain is wholly removed, and its powers seem not at all immediately impaired by the removal of the brain; yet we constantly see injuries of the brain impairing the functions of the spinal marrow. Of this apparent inconsistency, M. le Gallois justly remarks, that two facts well ascertained, however inconsistent they may seem, do not overturn each other, but only prove the imperfection of our knowledge.

Whichever of the disputed opinions, respecting the functions of the nervous system, we adopt, the foregoing phenomena seem to imply a contradiction. The experiments related in the following chapter point out still another instance of this apparent contradiction, and suggest the principle on which it as well as the others depend.

CHAPTER III.

On the Principle on which the Action of the Muscles of voluntary Motion depends, and the relation which they bear to the Nervous System.

We are now to consider how far the principle on which the action of the muscles of voluntary motion depends, and the relation which they bear to the nervous system, resemble those of the heart and vessels of circulation.

Exp. 34. By the application of stimulants to the spinal marrow of a newly-dead frog, strong and repeated contractions were excited in the muscles of the hind limbs, as long as the stimulants would produce the effect. On examination, the muscles of these limbs were found wholly deprived of their excitability. We should conclude from this experiment, that the power of the mus-

cles of voluntary motion is dependent on the nervous system; on the other hand, it is well known, that although all the nerves supplying the limbs of a frog be divided, and cut out close to the place where they enter the muscles, the latter still retain their excitability, which appears to be not at all less than when the nerves are entire; the fact which suggested to Haller the independence of the muscular power.

His opponents, however, objected to his inference, because although the division of the nerves may prevent the muscle from receiving more nervous power, it does not deprive it of that already bestowed on it, either constituting a necessary part of its power, or dispersed through its substance in nerves too small to be removed; and this objection appears to be greatly strengthened by the muscle soon losing its excitability after it is separated from the brain and spinal marrow, and those muscles, the function of which is supported by stimulants peculiar to themselves, being still supplied with nerves.

It appeared to me that this question could only be determined by some experiment capable of ascertaining whether the excitability of muscles is maintained by the influence they receive from the nervous system, or impaired as it is found to be by other stimulants; for if on the one hand it can be proved that the permanency of their excitability is unimpaired by cutting off all supply from the nervous system; and on the other, that the influence of that system exhausts it as other stimulants do, a doubt, I conceive, cannot remain respecting the nervous influence acting only as a stimulant to the muscular fibre, and the dependence of muscular power on the muscular fibre itself.

Exp. 35. The nerves of one of the hind legs of a frog were divided, and thus the limb wholly deprived of sensation and voluntary power. The mere division of the nerves of a muscle deprives it of the nervous influence, although the mere division of the nerves of a vital organ is not sufficient wholly to deprive it of its nervous influence. The proofs and cause of both facts will appear as we proceed. The skin was removed, and a stimulant

constantly applied to the muscles till no further contraction could be excited in them, which happened in twelve minutes. The corresponding muscles of the other limb were in the same manner subjected to the same stimulant. Here the muscles were subjected to the stimulant, while the influence of the nervous system was freely communicated to them; and as the animal, while the nerves are entire, by influencing the muscles to which the stimulant is applied, always endeavours to counteract its effect, the muscles were now exposed both to the effect of the artificial stimulant and that of the nervous power.

They lost their contractile power in ten minutes; and in a repetition of the experiment, they lost it in half the time required for exhausting those whose nerves had been divided.

It is evident from these experiments, that the influence of the nerves, so far from bestowing excitability on the muscles of voluntary motion, exhausts it like other stimulants. The excitability, therefore, is a property of the muscle itself. Yet all admit that these muscles are under the influence of the nervous system. It even appears from Experiment 34, that their power may be wholly destroyed by changes induced on that system. On the same principle we explain the seeming contradiction respecting the action of the heart and vessels. We have seen that their power exists as independently of the brain and spinal marrow, as that of the first muscles to which the artificial stimulant was applied, whose nerves had been divided; but, while the brain and spinal marrow retain their functions, and the connexion of nerves is entire, the heart and vessels, as well as the muscles of voluntary motion, may be influenced by agents acting through the nervous system. It is not difficult to account for these muscles being more copiously supplied with nerves than the heart, influenced as they are in all their usual functions through their nerves; while the heart is only occasionally excited through the nervous system, its usual stimulant being as immediately applied to it, as the artificial stimulant was to the muscles of the limb in the foregoing experiments, and acting as independently of the

nervous system. In like manner we understand why, when the connexion of parts is entire, the spinal marrow is subjected to the brain, although we know that its power is wholly independent of that organ, by finding it capable of all its immediate functions after the brain is removed.

In the experiments which have been laid before the reader, he cannot surely see any difference in the nature of the muscular power of the heart, and that of the muscles of voluntary motion, except their being fitted to obey different stimulants—a difference which, as might be expected, appears, from direct experiment, to exist in the two sides of the heart itself, the natural stimulant of the one being red, of the other black, blood.

It may here be objected, that in apoplexy the power of the muscles of voluntary motion is lost, while that of the heart is often little or not all impaired. Were such the fact, this objection would be unanswerable; but the author has repeatedly examined the muscles of voluntary motion in a state of apoplexy, both in warm and cold blooded animals, and found their excitability unimpaired. It is not their power, but the stimulant which excites them, that is lost in apoplexy. In this disease, the heart continues to contract, because its stimulant, the blood, is still supplied; the muscles of voluntary motion cease to contract, because their stimulant, the nervous influence, has failed.

The conclusions afforded by the foregoing experiments so far agree with those of Haller, that they prove the heart and other muscles to possess an excitability independent of the nervous power; but they prove, in opposition to the system of that great physiologist, that the heart is, equally with the muscles of voluntary motion, although in a different way, capable of being stimulated by that power.

In the report of the Institute of France, which has been laid before the reader, it is observed, "The adversaries of irritability have asked, why, if the nervous power bestows no power on the heart, is this organ supplied with nerves, and why is it subjected to the influence of the passions? Haller never gave any satis-

factory reply to these objections, but everything proves that he felt all their force." These objections, which it will soon appear no longer exist, we have seen, prevented Haller's doctrine of irritability from being generally admitted by physiologists, and at length led M. le Gallois to suppose that he had wholly refuted it, in which we have seen, from direct experiment, both he and the Committee of the Academy of Sciences were deceived.

We may, it appears to me, trace the subject farther. The reader has seen that the spinal marrow is capable of exciting the muscles, and we shall find it capable of its other functions, independently of the brain; yet in the sensitive system we constantly see the spinal marrow influenced through the brain. Thus the excitability of the spinal marrow, as far as relates to this system, bears the same relation to the brain, which that of the muscles bears to the spinal marrow. Even M. le Gallois, although his experiments lead to an opposite conclusion,[1] observes, that the brain seems to act on the spinal marrow as the latter does on the parts it animates. We know the functions peculiar to the brain, by observing what functions are lost by its removal, those of the sensorial power. It would appear, therefore, that as the muscular obeys the nervous, the nervous obeys the sensorial system.

In a future part of this Inquiry, the author will endeavour, by experiment, to point out with more precision than has hitherto been done, the line of distinction between the sensorial and nervous functions, which appears to be very different from that assumed by M. le Gallois;[2] and to ascertain the relation which they bear to each other.

[1] He infers from his experiments that the power of the heart ceases on the destruction of the spinal marrow, but that that of the spinal marrow remains after the destruction of the brain.

[2] See chap. 11.

CHAPTER IV.

On the comparative Effects of Stimulants, applied to the Brain and Spinal Marrow, on the Heart and Muscles of Voluntary Motion.

In making the experiments related in the preceding chapters, it was evident, that although the muscles of involuntary are, equally with those of voluntary motion, subject to the effects of stimulants applied to the brain and spinal marrow, the laws which regulate these effects on the two sets of muscles are very different. I have endeavoured by the following experiments to ascertain in what this difference consists.

Exp. 36. Part of the cranium of a rabbit was removed, and a wire was passed in various directions through the brain, which, so destitute of sensibility is the brain, occasioned no pain. The muscles of voluntary motion could not in this way be at all excited, except when the wire approached those parts of the brain from which the spinal marrow and nerves originate. Some of the muscles of voluntary motion were then excited. The whole of the upper and anterior part of the brain was sliced off, without giving any pain, or affecting the muscles of voluntary motion. The instrument only excited their action when it approached the source of the spinal marrow and nerves.

Exp. 37. A rabbit was killed in the usual way; part of the cranium was then removed, and the thorax laid open. The heart was found beating regularly. By passing a wire through the brain in any direction, the beats of the heart were accelerated and rendered stronger. The author could not perceive that this effect was produced more powerfully when the wire was directed towards the source of the nerves, than when any other direction was given to it, provided it passed through an equal portion of the brain. When an instrument was merely pressed on the sur-

face of the brain, the effect was similar. When a pair of scissors, or any other thing of larger bulk than the wire, was passed into the brain, the effect on the heart was greater than from the wire. It was still greater when the brain was wounded rapidly in many directions.

Exp. 38. Part of the cranium of a rabbit was removed, and after passing an instrument through the brain in various directions near to the origin of the nerves, which excited strong spasms in the muscles of voluntary motion, the blood being absorbed by a sponge, spirit of wine was applied to the surface of the brain, and dropt into its substance, without causing any pain, or at all affecting the muscles of voluntary motion. The upper part of the brain was then wholly removed, and spirit of wine applied to it, but neither signs of pain nor spasms of the muscles of voluntary motion ensued.

Exp. 39. Another rabbit was killed in the usual way, and part of the cranium removed. The thorax was then laid open, and the heart found beating regularly. Spirit of wine was now applied to the surface of the brain, by which the frequency and force of the heart's beats were immediately increased. Several cuts were then made in the brain, and the spirit of wine dropped into them, by which, whatever the direction of the cuts, the action of the heart was increased in a much greater degree. Spirit of wine increased the action of the heart more than any mechanical injury, which never produced the strong and irregular action in this organ, which it does in the muscles of voluntary motion.

This experiment was repeated with a watery infusion of opium instead of spirit of wine; the result was in all respects the same, except that the action of the heart was less increased than by the spirit of wine.

Exp. 40. Under the term brain I mean to include the cerebellum as well as the brain properly so called. From many trials on the newly-dead rabbit made to ascertain the point, I could not perceive that the heart is more or less affected either by chemical

or mechanical stimulants applied to the cerebellum than to the brain; nor are the muscles of voluntary motion affected by wounding the cerebellum, except we approach the source of the spinal marrow and nerves. In some of my experiments I thought that stimulants applied to the cerebellum affected the action of the heart rather more powerfully than when applied to the brain; but this was contradicted by other experiments.

Exp. 41. The head of a rabbit was cut off close to the occiput. For some time the trunk and limbs were affected with strong spasms. The slightest touch of a wire applied to the cut end of the spinal marrow, after the spasms had subsided, immediately excited the action of some of the muscles of voluntary motion. The strongest spirit of wine and watery infusion of opium were applied to it, without producing the least effect on those muscles. The application, however, of stronger chemical stimulants, the undiluted nitric and muriatic acids, threw the muscles of the fore-legs into powerful contractions. Was this by the mechanical effect of the corrugation they occasion? A repetition of this experiment gave the same results.

In a rabbit killed in the usual way, the reader has seen that both spirits of wine and a watery infusion of opium applied to the spinal marrow increase the action of the heart.[1]

Exp. 42. It was repeatedly found both in newly-dead rabbits and frogs that, after all stimulants applied either to the brain or spinal marrow had ceased to produce any excitement in the muscles of voluntary motion, both chemical and mechanical stimulants so applied still increased the action of the heart; the former more than the latter.

Exp. 43. I tried in every possible way, both by mechanical and chemical stimulants, to excite, through the brain or spinal marrow of newly-dead rabbits and frogs, any irregular action in the heart which is so readily excited in the muscles of voluntary motion, but could not. Nor could I by sedatives, (such substances as tend to allay action,) applied to any part of the nervous system, occa-

[1] Exp. 13.

sion any irregular action in this organ. Its action was rendered quicker or slower, more or less frequent, stronger or weaker, but never irregular. Irregular action was never excited in the heart, except when its power was nearly destroyed by crushing the brain or spinal marrow.

Exp. 44. I found from many trials both on newly-dead rabbits and frogs, that the excitement of the muscles of voluntary motion took place chiefly at the time the stimulant was applied to the brain or spinal marrow. It was generally necessary to move the instrument, thus applying it to a new surface, in order to support the effect. It is known that in the living animal repeated contractions of the muscles of voluntary motion will sometimes continue for a considerable time when an extraneous body remains in the brain.

The increased action of the heart could generally be observed as long as the stimulant, whether chemical or mechanical, was applied, unless it was of a nature to produce the sedative after the stimulant effect. The sedative effect is so far from always being the consequence of previous excitement, as many physiologists have supposed, that spirit of wine and mechanical stimulants, which produced no sedative effect, but continued to stimulate the heart as long as they were applied, produced a much greater degree of excitement than tobacco, whose slight stimulant effect was quickly succeeded by a powerfully sedative one.

It appears from these experiments, that chemical stimulants, applied to the brain and spinal marrow, exert a greater power over the heart than mechanical stimulants, while the latter exert a far greater power over the muscles of voluntary motion than chemical stimulants; that both chemical and mechanical stimulants, applied to the brain and spinal marrow, excite the heart after they cease to produce any effect on the muscles of voluntary motion; that stimulating every part of the brain and spinal marrow affects the action of the heart, while the muscles of voluntary motion are only excited by stimulants applied to the parts

of those organs from which their nerves originate; that stimulants applied to the brain and spinal marrow never excite irregular action of the heart, while nothing can be more irregular than the action they excite in the muscles of voluntary motion; and that their effect on these muscles is felt chiefly on their first application, but continues on the heart as long as the stimulant is applied. These differences in the effects of stimulants applied to the brain and spinal marrow on the muscles of voluntary and those of involuntary motion, must be explained before we can be said to understand the relation which subsists between the nervous system and the heart.

It appeared to me probable, from many experiments, that the cause of chemical stimulants, applied to the nervous system, producing a greater effect on the heart than mechanical stimulants do, is, that the former from their nature act on a larger portion of the brain and spinal marrow. If this opinion be correct, the mechanical stimulant will be rendered the most powerful by confining the chemical to a smaller space than the mechanical stimulant occupies.

Exp. 45. In newly-dead frogs and rabbits, the author applied to various parts of the brain and spinal marrow, and particularly to those parts from which the nerves originate, minute portions of strong spirit of wine, without at all influencing the action of the heart. When these small portions were applied to a great many parts, the heart began to beat more frequently. This, of course, was much the same thing as at once applying the spirit of wine to a larger part.

It appears from the foregoing experiments, that mechanical stimulants, applied to any considerable portion of the brain or spinal marrow, increase the action of the heart; and from the following, that we cannot affect the heart by such stimulants confined to any small part of either of these organs.

Exp. 46. In a rabbit killed in the usual way, different small parts of the brain were wounded with a wir e, particularly all those parts near which the nerves of the heart appear chiefly to

originate; but the motion of this organ remained uninfluenced, while at the same time passing the wire through any considerable portion of the brain immediately accelerated it.

Exp. 47. The cervical part of the spine of a newly-dead rabbit was laid open, and a wire was repeatedly passed transversely through the spinal marrow, without at all affecting the motion of the heart; but on the wire being passed longitudinally, so as to bring it into contact with a larger portion of the spinal marrow, the motion of the heart was immediately accelerated. On the same principle, when the wire was made to wound many minute portions of the brain and spinal marrow in quick succession, the action of the heart was increased. In another newly-dead rabbit, the spinal marrow was divided close to the head, without at all affecting the heart.

Mr. Clift, in an account of a repetition of some of my experiments, in which the carp was the subject, published in the *Philosophical Transactions for* 1815, observes, that on dividing the spinal marrow at the occiput, the action of the heart was greatly accelerated for a few beats; but he divided the spinal marrow while the animal retained the power of the muscles of voluntary motion, which never fail to be called into action by wounding it, and the action of which, by increasing the flow of blood, always accelerates the motion of the heart.[1]

Thus we see that neither chemical nor mechanical stimulants, applied to the brain and spinal marrow, affect the action of the heart, unless they make their impression on a considerable portion of these organs. In the various experiments just related, every part of them was stimulated individually, without the action

[1] Mr. Clift, on repeating my experiment, in which the spinal marrow was destroyed by a small wire, found the same result in the carp, which I had found in rabbits and frogs. Mr. Clift did not ascertain whether the circulation continued after the destruction of the spinal marrow; but from this occasioning little or no diminution in the action of the heart, we cannot doubt the continuance of the circulation

of the heart being influenced; and the stimulant being the same, the force with which it acted on this organ was always proportioned to the extent of surface to which it was applied. I could not find that it was of any importance what part of the brain or cerebellum was stimulated. Even stimulating the surface alone, either mechanically or chemically, immediately increased the action of the heart. The muscles of voluntary motion, on the contrary, it appears from the foregoing experiments, are wholly insensible to stimulants applied to the brain, except near the origin of the nerves and spinal marrow.

Another circumstance, which appears to be of great importance in tracing the cause of the different effects of stimulants applied to the brain and spinal marrow on the muscles of voluntary and involuntary motion, is, that the heart obeys a much less powerful stimulant than the muscles of voluntary motion do. It appears from what has been said, that only the most powerful chemical stimulant affects them, while all that were tried readily influenced the action of the heart. Mechanical stimulants which, by bruising and dividing the parts, occasion the greatest possible irritation, are best fitted to excite the muscles of voluntary motion. Chemical stimulants, indeed, from their effects on the heart, we should consider the most powerful. But their greater effect on this organ is readily explained by the influence of stimulants applied to the brain and spinal marrow on the heart, being proportioned to the extent of surface to which they are applied. It is evident that the stimulant can be applied to a greater extent of surface in the fluid than in the solid form. When the effect of the mechanical agent is rendered extreme and extensive, we find its influence on the heart far greater than that of any chemical agent. From experiments related in the second chapter of this Part it appears that suddenly crushing any considerable portion of the brain or spinal marrow instantly and wholly for the moment, destroys the power of the heart, which cannot be done by a chemical agent, if we except electricity, in whatever manner, and however extensively applied.

The conclusions then at which we arrive are, that the heart is excited by all stimulants applied to any considerable part of the brain or spinal marrow, while the muscles of voluntary motion are only excited by intense stimulants applied to certain small parts of them.

These facts being ascertained, the other differences observed in the effects of stimulants applied to the brain and spinal marrow, on the heart and muscles of voluntary motion, are easily explained.

Irregular action of a muscle arises from stimulants acting partially, or at intervals, on its nerves, or on the particular part of the brain or spinal marrow from which its nerves arise. But very partial action of a stimulant on the brain and spinal marrow, we have just seen, is incapable of exciting the heart, and while the stimulant is applied to any part of these organs, as all parts of them seem equally to influence the heart, it cannot act upon it interruptedly, as an instrument does on the muscles of voluntary motion when it is moved from place to place in the brain.

When in the newly-dead animal the instrument is kept still after it is introduced into the brain, the action of the muscles of voluntary motion ceases; its merely being in contact with the parts of the brain which excite these muscles, not being sufficient to call them into action. As the muscles of voluntary motion feel the impression made on a very small part of the brain only, in proportion as this part is small, the impression must be great to affect them; and this impression of course is greatest while it is bruising or wounding the part; but the heart, which is influenced through all parts of it, though not very powerfully through any one, feels all the impressions made on this organ, provided they are made on a sufficiently extensive portion of it: thus, within certain limits, as long as the instrument remains in the brain, its stimulant effect on the heart continues.

It is true, that although the heart is only influenced by agents applied to a large portion of the brain or spinal marrow, we may conceive them so applied as to produce irregular action in it, and

we find that certain irritations of the nervous system have this effect. But it is evident, that the heart not being subject to stimulants whose action is confined to a small portion of these organs, and being nearly equally affected through all parts of them except the lumbar portion of the latter, must render it much less subject to irregular action; which may be one of the final causes of the heart, whose regular action is of such importance in the animal economy, being made subject to the whole, and not to any one part of those organs; [1] and readily accounts for our not being able to produce irregular action in it, in the experiments above related.

What has been said also explains why those who have endeavoured to influence the heart, by stimulating the parts of the brain from which its nerves seem chiefly to originate, have failed. When indeed the source of the nerves of the heart is considered, it will be found to derive its nervous influence from every part of the nervous system, and not very remarkably from any one part; a circumstance which well corresponds with the result of the foregoing experiments.

From the same facts it is evident, why the heart is stimulated through the brain and spinal marrow, after their power is so far weakened as no longer to convey the effect of the stimulant to the muscles of voluntary motion. As these obey stimulants applied to only one part of those organs, if the change in this part is not sufficiently strong to produce the effect, or if its powers have been exhausted, it cannot be assisted by any other. Thus it appears from experiment, that a blow which affects the brain generally, without materially injuring it, produces comparatively little effect on the muscles of voluntary motion, because no one part suffers greatly; but it produces a great effect on the heart, because it feels the sum of all the im-

[1] In the course of this Inquiry, another and apparently more important reason will appear, why it is necessary that the heart should be subject to the influence of every part of the brain and spinal marrow.

pressions. The nervous system, therefore, may be so far exhausted as not to admit of the vivid impressions necessary to excite the muscles of voluntary motion, and yet capable of those which influence the heart. When the effects of the blow are extreme, it then of course affects both classes of muscles.

CHAPTER V.

On the Sources and Nature of the Powers of Circulation. [1]

It is remarkable that, notwithstanding the great importance of the circulation in the animal economy, the length of time which has elapsed since its discovery, and the constant attention it has obtained, there is hardly any department of physiology respecting which there appears to be greater uncertainty and contrariety of opinion, than the sources and the nature of the powers on which this function depends. I propose in the following chapter, by comparing the principal facts on the subject, and by such additional experiments as seem still to be required, to endeavour to determine these points. Much has lately been written, and many experiments have been made with this view, and it has become customary to look for the causes which support the circulation to other sources in combination with the powers of the heart and blood-vessels.

It has been supposed that what has been called the resilience of the lungs, that is, their tendency to collapse, by relieving the external surface of the heart from some part of the pressure of the atmosphere, is a principal means of causing it to be distended with blood, the whole weight of the atmosphere acting on its internal surface through the medium of the blood which is thus propelled from the veins into its cavities; and in this way it has

[1] Philosophical Transactions for 1831.

by some been supposed that the motion of the blood through the whole of the venous part of the circulation is maintained. A similar effect has been ascribed to the act of inspiration, which it is evident must operate on the same principle: and this opinion has even been sanctioned by the Report of a Committee of the Royal Academy of Sciences of Paris,[1] and in this country by men whose authority is deservedly high; and the effect of these causes, it is asserted, is increased by the elastic power of the heart itself.

However successfully such opinions might be combated by reasoning on the data we already possess, as direct experiment is the most simple as well as decisive way of determining the question, as reasoning on physiological subjects has so often deceived, and the experiments may here be made on the newly-dead animal, and consequently without suffering of any kind, I have thought it better that the point should be determined in this way, especially as it is by experiments, which at first view seem to countenance the foregoing opinions, that their supporters attempt to establish them, with the effect, as it appears to me, of withdrawing the attention from the powers on which the circulation actually depends, and introducing considerable confusion respecting a question so immediately connected with the phenomena and treatment of disease.

With a view, therefore, to submit the foregoing opinions to this test, the following experiments were made, in which Mr. Cutler was so good as to assist me.

Exp. 48. A rabbit was killed in the usual way for the table by a blow on the occiput, and the chest opened on both sides so as freely to admit the air. The lungs were then inflated eight or ten times in the minute by means of a pipe introduced into the trachea: the circulation was found to be vigorous. On laying bare one of the femoral arteries, it was observed to pulsate strongly; and on wounding it, the blood, of a florid colour, indi-

* Report on Sir David Barry's paper, by Baron Cuvier and Professor Dumeril.

cating that its colour had undergone the proper change in its circulation through the lungs, gushed out with great force; and on introducing the hand into the thorax, the heart was found to be alternately distended and contracted as in the healthy circulation.

Exp. 49. All the vessels attached to the heart in the newly-dead rabbit being divided, and the heart removed, it was allowed to empty itself. Its contractions continued to recur, and in their intervals it assumed a perfectly flat shape, proving that the elasticity of the heart in this animal is so small that it cannot even maintain the least cavity after the blood is discharged.

It appears from these experiments that the circulation was vigorous when none of the causes to which the motion of the blood in the veins have been ascribed existed. In the first experiment the chest being freely opened on both sides, so that the play of the lungs on inflating them could be seen, all effect on the heart, as far as related to the motion of the blood in the veins either of the resilience of the lungs or the act of inspiration, was evidently prevented; and in the second, it was proved that no sensible elasticity of the heart existed; yet, while artificial respiration was performed, we could perceive no abatement in the vigour of the circulation.

It is to be observed, that all the foregoing causes can act only in one way in promoting the circulation, namely, by giving to the heart the power of suction; that is, by producing a tendency to vacuum in its cavities, in consequence of which the pressure of the atmosphere propels the blood from the veins into them, that of the arteries being prevented from returning to the heart by the valves at their origins. But all, as far as I know, who have either made experiments with a view to prove the supposed effect of these causes on the circulation, or who have sanctioned the inferences from such experiments, have overlooked the circumstance, that the veins being tubes of so pliable a nature that when empty they collapse by their own weight, whatever may be said of the effect of such causes in favouring a horizontal or descending mo-

tion of the blood, it is impossible that an ascending motion could be produced in them on the principle of suction. As far as the heart may possess any such power, its tendency must be to cause the vessel to collapse, not to raise the fluid it contains.

That the resilience of the lungs,[1] as far as they possess this property, and the act of inspiration, tend to dilate the heart and large vessels within the chest, is evident; but the former is very trifling, if it exist at all, except as far as it depends on the mere weight of the lungs; and the latter in common breathing is little more efficient, although the effect of respiration on the brain, when any part of the cranium is removed, sufficiently attests that it has a certain effect. When the breathing is so laborious as essentially to influence the circulation, it evidently tends to derange the regular flow of the blood towards the heart, inspiration of course acting interruptedly; whereas it is only necessary to inspect the chest of any of the more perfect animals immediately after death, and while artificial respiration is being performed, provided death has not been caused by great loss of blood, or an extreme and instantaneous impression on the nervous system, to see that the blood flows uniformly towards the heart, with no interruption but that which the contractions of the heart itself occasion.

The elasticity of the heart is greater in some animals than in the rabbit; but it is in all cases very inconsiderable. The heart of the tortoise is the most elastic I have examined; yet even it, when the blood is no longer propelled into it—for the alternate contractions and relaxations of the heart continue after it is separated from the body—may be compressed, during the intervals of

[1] This power cannot operate in many of the lower classes of animals, or in the fœtal state in any class; and in the perfect animal of the higher classes its effect is very inconsiderable. It appears, from an experiment related in the 111th page of Dr. Carson's Treatise, that even in the bullock it is not equal to the pressure of a column of water of ten inches. It does not indeed appear from the experiments of Dr. Carson, how much the effect he observed depends on what he calls the resilience, or merely on the weight of the lungs.

its contractions, by a force not sensibly greater than is sufficient to compress other muscles in a state of relaxation. Besides, the auricles possess little or no elasticity; and whatever the elasticity of the ventricles may be, it can have no effect on the blood in the veins, because they receive their blood from the auricles, which are contracting during the dilatation of the ventricles. To these statments it may be added, that in many of the inferior animals the foregoing supposed causes of the venous part of the circulation evidently have no existence, and that, with the exception of the elasticity of the heart, they have no existence in the fœtal state in any.

We have just seen, from direct experiment, that, in the perfect animal, the circulation of the blood goes on as usual when none of these causes can operate.

I shall now take a rapid view of the facts which, as far as I am capable of judging, leave no room for doubt respecting the sources of the power on which this function depends.

It is so evident to those in the least acquainted with the animal economy that the contractile power of the heart is one of the chief of these sources, that it would be superfluous to enumerate the proofs of it; yet, such is the love of singularity, that even this position has been denied. The opposite error, however, is the more common; and not a few have ascribed, and even still do ascribe, the motion of the blood throughout the whole course of circulation to the contractile power of the heart alone, although it would not be difficult to prove that to drive the blood through one set of capillary vessels, and still more through two or three sets of such vessels—for in man himself, in one important part of the circulation, it is carried through two, and in some animals through three, sets of capillaries before it returns to the heart,—I say it would not be difficult to prove that to drive it through one set of capillaries, at the rate at which the blood is known to move, would require a force capable of bursting any of the ves-

sels. But here, as in the former instance, it is better to appeal to the evidence of direct facts than to any train of reasoning; and there is no want of such facts to determine the point before us. The most decisive is, that the motion of the blood in the capillaries continues long after the heart has ceased to beat, and the animal in the common acceptation of the term is dead, and even in the warm-blooded animal, if it be favourably circumstanced, for several hours, and it is not for some time sensibly affected by the heart ceasing to beat; nor does this arise from some imperceptible impulse still given by the heart, because, when all the vessels attached to this organ are secured by a ligature and the heart cut out, the result is the same.[1]

That the circulation in the capillary vessels is independent of the heart, may be shown by various other means. On viewing the motion of the blood in them with the assistance of the microscope, it may generally be observed that it is moving with different degrees of velocity in the different vessels of the part we are viewing, frequently more than twice as rapidly in some than in others. Were the motion derived from a common source, this could not be the case. It is impossible, in the motion of the blood in the capillaries, in the least degree to perceive the impulse given by the beating of the heart, which causes the blood in the arteries to move more or less *per saltum*, the motion of the blood in the former being uniform as long as they retain their vigour, and the necessary supply of blood is afforded from the larger vessels. I have found by experiments very frequently repeated,[2] that the motion of the blood may be accelerated or retarded in the capillaries by stimulants or sedatives, acting not through the medium of the heart, but on these vessels themselves. Nay, so little effect has the action of the heart on the motion of the blood in the capillaries, that when the power of the capillaries

[1] Experimental Inquiry, Experiments 66 and 67. Third Edition.

[2] Experimental Inquiry, Part III. Chap. II., and the Introduction to the second part of my Treatise on Febrile Diseases. Fourth Edition.

of a part is suddenly destroyed by the direct application of opium to them, the motion of the blood in them instantly ceases, although the vigour of the heart and that of every other part of the sanguiferous system be entire.[1]

If the circulation in the capillaries be thus independent of the heart, it is evident that the influence of that organ cannot extend to the veins. On comparing the whole of the foregoing circumstances, is it not a necessary inference that the motion of the blood in the veins, like that in the capillaries, depends on the power of these vessels themselves? But that we may not trust to any train of reasoning, where it is possible to have recourse to direct proof, I made the following experiment, with the assistance of Mr. Cutler.

Exp. 50. In the newly-dead rabbit, in which the circulation was maintained by artificial respiration, the jugular vein was laid bare for about an inch and a half; a ligature was then passed behind the part of the vessel nearest to the head, and the animal was so placed that the vein was brought into the perpendicular position, the head of the animal being undermost, so that it was necessary for the vein, in conveying the blood to the heart, to convey it perpendicularly against its gravity. The ligature, which was placed at what was now the lowest part of the exposed portion of the vein, was suddenly tightened, while Mr. Cutler and myself observed the vessel. The blood in the part of the vein between the ligature and the heart was instantly expelled, as the transparency of the vessel enabled us to perceive. The vessel itself wholly collapsed, proving that all its blood had entered the heart, so that to a superficial view there seemed to be no vessel in the part where a large dark-coloured vein had just before appeared. In the mean time, on the other side of the ligature, the vein had become gorged with blood.

In the foregoing experiment we see the blood rising rapidly against its gravity, where all causes external to the vessel on

[1] Part III. of this Inquiry, and the Introduction to the second part of my Treatise on Febrile Diseases. Fourth Edition.

which the venous part of the circulation has been supposed to depend, had ceased to exist, and the *vis à tergo* was wholly destroyed by the ligature.

By a similar experiment, the power of the arteries in propelling the blood may also be demonstrated.

Exp. 51. In a newly-dead rabbit, the circulation being supported by artificial breathing, the carotid artery was laid bare for about an inch and a half. The animal was so placed as to keep the vessel in the perpendicular position, the head being now uppermost. A ligature was passed behind that part of the vessel which was next the heart, and Mr. Cutler and myself observed the vessel at the moment the ligature was tightened. The artery, of course, did not collapse as the vein had done in the preceding experiment; but the blood was propelled along the vessel, so that it no longer appeared distended with it. It was at once evident, from the change of appearance in the vessel, that the greater part of the blood had passed on in a direction perpendicularly opposed to its gravity. It is worthy of remark, that the blood of the artery was propelled neither so rapidly nor so completely as that of the vein, the cause of which will be evident in the observations I am about to make on the nature of the function and powers of these vessels.

When the whole of the preceding facts are considered, it will, I think, be admitted that the circulation is performed by the combined power of the heart and blood-vessels themselves, and that no auxiliary power is necessary for its perfect performance. Here, as in other cases, the more we study the operations of nature, the more direct and simple we find them. The resilient power of the lungs and elasticity of the ventricles of the heart, as far as they exist, favour the free entrance of the blood into these cavities, an office adapted to the feebleness of such powers, which, in many animals, we have seen, have no existence. Their operation is similar, but much inferior, to that of the elastic power of the arteries, by which the ingress of the blood suddenly impelled into them by the contraction of the heart is rendered more free than

it would have been, had these vessels, like the veins, tended to collapse in the intervals of their contractions. Had the blood flowed into them in a continued stream, and been carried through them by their own powers alone, their elasticity would evidently have impeded, not promoted, the circulation through them. It could only have encumbered the effect of those powers. Thus the veins, in which these conditions obtain, are so pliable that they collapse by their own weight, and hence it was that in the preceding experiments the vein carried on its blood so much more rapidly and completely, than the artery, which felt the encumbrance of its elastic, that opposes the free action of its muscular, coat; and the want of the impulse it receives from the heart, that at once assists in propelling its blood, and through the blood stimulates the vessel itself; while the action of the vein was perfect; because it possessed all its usual powers, which reside in itself alone.

It only remains for us to inquire into the nature of the power by which the heart and blood-vessels maintain the circulation. Respecting the nature of the power of the heart there cannot be two opinions. It is evidently a muscular power. The structure of its parietes is similar to that of other muscles, and they obey all the usual laws of the muscular fibre.

Is the power of the vessels of the same nature? This is a question which has frequently been discussed. The chief arguments which have been adduced in favour of the affirmative are:—the nature of their function; the fibrous appearance observed in some of the vessels, which is more evident in some other animals than in man; and the minuteness of most of the vessels, which, if they are muscular, accounts for the difficulty with which the muscular structure is detected in them. The chief arguments against the muscularity of the vessels have been, that they could not be made to obey an artificial stimulant in the way that the heart and other muscles are found to do, and that their chemical analysis gives no evidence of fibrin. Of the latter of these objections Dr. Young observes, that a part may be muscular although

it does not contain fibrin, and refers in support of this opinion to the crystalline lens. The former of these objections no longer exists, the vessels having been found to obey both stimulants and sedatives as readily as parts more evidently muscular. It appears from many experiments related in my Treatise on the Vital Functions, that the action of the capillary vessels is as easily influenced both by stimulants and sedatives as the heart itself;[1] and although the larger vessels are not so easily excited artificially as the heart and muscles of voluntary motion, yet several physiologists have succeeded in exciting them by mechanical agents. But there is another argument in favour of the muscularity of the vessels, which, I think, may be regarded as no less powerful. I endeavoured, in the papers which I had the honour to present to the Royal Society, and which appeared in the Philosophical Transactions for 1815, to ascertain the relation which the heart bears to the nervous system, which is different from that of the muscles of voluntary motion. It appears from the facts there adduced, that this organ is not only like the muscles of voluntary motion, independent of that system, although capable of being influenced through it either by means of stimulants or sedatives, and that even to the instantaneous destruction of its power; but that it equally obeys either set of agents, whether applied to the brain or spinal marrow, and to whatever part of these organs applied, provided it be to a part of considerable extent; while the muscles of voluntary motion obey no stimulus acting through the nervous system, unless it be applied to their nerves themselves, or to the particular parts of that system from which their nerves originate. Now we have seen from repeated experiments that the vessels bear the same relation to the nervous system as the heart does, their power being independent of this system, but equally with the heart capable of being influenced by either stimulants or sedatives applied to any considerable part either of the brain or spinal marrow, and that even to the instantaneous destruction of their power. They in all respects bear the same relation to the nervous system with the

[1] Experimental Inquiry, Part II. Chap. xi. and Part III. Chap. ii.

heart—the strongest presumption that their power is of the same nature.[1]

From the various facts stated or referred to in the foregoing paper, the following inferences appear to be unavoidable:—That the circulation is maintained by the combined power of the heart and blood-vessels; and that the power of both is a muscular power.

CHAPTER VI.

Some additional Facts and Observations respecting the Relation which subsists between the Nervous and Muscular Systems in the more perfect Animals, and particularly respecting the Function of the Ganglions.

We have seen that the power both of the muscles of voluntary and involuntary motion is independent of the nervous system, yet in both equally capable of being influenced by it, the nervous influence being the constant stimulant in the functions of the former class of muscles, and an occasional stimulant in those of the latter, which in their usual functions are excited by stimulants peculiar to themselves; but that these classes of muscles are influenced by it in very different ways, each of the muscles of voluntary motion being under the influence of no part of the brain or spinal marrow but the particular part from which its nerves arise; while each of the muscles of involuntary motion is under that of every part of these organs, from the upper surface of the brain and cerebellum to the lowest portion of the spinal marrow.

In subsequent papers, published in the Philosophical Transactions, and in the former editions of the present Inquiry, I en-

[1] Philosophical Transactions for 1833.

deavoured to trace the final cause of this arrangement; and found that the functions, with the exception of the circulation, to which the muscles of involuntary motion administer, namely, secretion and the assimilating processes, require for their due performance the united influence of every part of the brain and spinal marrow; while the muscles of voluntary motion are concerned in no function but that of giving motion to the different parts of the body to which they are attached, and consequently have no direct influence on the functions of life.

From the whole of the experiments on which is founded the view I have thus been led to take of the functions of the nervous system, and its relation to the different classes of muscles, it would appear that the brain and spinal marrow are the only active parts of that system; the nerves, whether cerebral or ganglionic with their ganglions and plexuses, being only the means of conveying, and, where the organs, the functions of which require the influence of every part of the brain and spinal marrow, are concerned, combining the influence of the various part of these organs.

But as this inference is less direct than the other inferences arrived at in the foregoing publications, and as very different opinions have been maintained both by preceding and subsequent physiologists, it appears necessary to review this part of the subject with more care, particularly as our view of many of the phenomena, both of health and disease, must be essentially modified by it. This, therefore, is the first question I propose to consider.

The various parts concerned in the functions of the living animal may be divided into active and passive,—those in which the power resides, and those which only obey that power, but which are equally essential to the function. Thus the belly of a muscle is the active—the tendon, the passive part of the organ. In the production and application of the bile, synovia, and other fluids prepared for the purposes of the animal economy, the gland is the active, the ducts, as far as the peculiar office of the organ

is concerned, the passive part of that organ; and I know of no way of ascertaining which is the active and which the passive part of any organ, but by observing which obeys the other in the function of the part.

It has been pretty generally admitted with respect to what may be called the cerebral part of the nervous system, in contradistinction to the ganglionic, that is, certain parts of the brain and spinal marrow, with the nerves immediately proceeding from them, that the former are the active, the latter the passive parts of these organs; because we find that the power of the nerves is always proportioned to the excitement of the brain and spinal marrow, as that of the tendon is proportioned to the excitement of the belly of the muscle. Even this position, however, has by some been controverted, and it has been maintained that the nerves themselves in part supply the influence on which their functions depend.

But in whatever manner this part of the question, which it will be necessary to consider more particularly, may be disposed of, there is certainly, from the more complicated structure of the parts, better reason for regarding the ganglionic nerves with their ganglions and plexuses, as active in the formation of nervous influence, than the simple nerve which connects the cerebral mass with the muscle which it excites; and it has consequently been the opinion of many physiologists that the ganglionic system is concerned in the production of this influence, and some have gone so far as to regard it as independent of the brain and spinal marrow, and therefore the only source of the power of its own nerves.

As soon as it was found that the organs supplied with ganglionic nerves obey every part of the brain and spinal marrow, it was necessary to abandon the latter opinion, and we could see a reason for the complicated structure of the ganglionic system, independently of its supplying any part of the nervous influence. As each of the vital organs is influenced by every part of the brain and spinal marrow, some apparatus capable of

combining the influence of all these parts is evidently necessary, and none apparently could be better fitted for the purpose than that system, which, both by its ganglions and plexuses, and the frequent anastomoses, if I may use that expression, of its nerves, seem even at first view intended to combine the power of the various parts from which it receives nerves; and when those proceeding from the ganglions and plexuses are found by direct experiment to convey the influence of all those parts, and this fact is compared with the preceding positions, the inference that to combine and convey the influence of the various parts of the brain and spinal marrow are the exclusive functions of the ganglions, their plexuses and nerves, appears almost unavoidable.

We here have a proof that the organs supplied by ganglionic nerves obey the influence of all parts of the brain and spinal marrow, and consequently that in the ganglionic nerves is combined and conveyed the influence of all those parts; and it is contrary to what we observe of the simplicity of the operations of nature that there should be another source of that influence.

In another fact we find an additional objection to such a supposition, for it appears from many experiments related in the preceding parts of this Inquiry, that exactly in proportion as we increase or impair the power of the brain and spinal marrow, the functions of the ganglionic nerves are increased or impaired; still pointing out the brain and spinal marrow as the active, and the nerves with their ganglions and plexuses as the passive parts of the system; and these observations come with the more weight, because those who have maintained that the ganglions supply nervous influence, have not even pretended to support their opinion by any facts directly bearing on the point.

If, however, it also appears from direct experiment that the ganglions and plexuses are capable of influencing the power of the ganglionic nerves, independently of any change induced on the brain and spinal marrow, however improbable the fact

may at first sight appear, we must admit that there is in the former organs an additional source of the power possessed by those nerves.

It has been found that the action of the heart is immediately influenced by agents, whether stimulants or sedatives, affecting any considerable part either of the brain or spinal marrow.[1] Can its action, in like manner, be affected by agents making their impression on the ganglions and plexuses? For the purpose of determining this point, the following experiment was made, in which Mr. Cutler, and Mr. Field, the well-known veterinary surgeon, were so good as to assist me, Mr. Field performing the operative part.

The heart, we have seen, continues to obey the effect of agents, whether stimulants or sedatives, applied to the brain and spinal marrow, for a certain time after what we call death, that is, the removal of the sensorial powers; and this time is much prolonged if the circulation be maintained by inflating the lungs at proper intervals; for in all modes of death, except where the nervous system is so powerfully and suddenly impressed as almost immediately to destroy all the functions, the nervous as well as the muscular functions for a considerable time survive the removal of the sensorial powers;[2] and the newly dead is on several accounts a better subject than the healthy animal for such experiments as the following, although the result is still more satisfactory if the animal can be so prepared as to destroy the sensibility as far as the experiment is concerned, without so completely destroying it as to interrupt respiration;[3] because it is impossible in several essential respects by artificial respiration to imitate the natural process, and consequently to support the circulation

[1] Papers in the Philosophical Transactions for 1815, and preceding parts of this Inquiry.

[2] Ibid.

[3] It will appear, as far as I can judge, from what is said hereafter, that in the more perfect animals, respiration is as much a function of volition as the motion of a limb, and consequently ceases when the sensibility is wholly destroyed. See the first and sixth chapters in this volume.

and the functions which depend on it so effectually as by that process. I shall have occasion, in another part of this volume, to point out the circumstances in which artificial necessarily differs from natural respiration.

Exp. 52. Mr. Field at my request partially divided the spinal marrow near the head in an ass in such a manner as to destroy the sensibility, as far as the experiment was concerned, but not to interrupt the respiration,[1] thus bringing the animal into the best possible state for the experiment. It lay as still, and suffered as little during it, as an animal quite dead in the usual sense of the word, while the circulation was more perfect than it could be under any artificial inflation of the lungs. In another respect, the state of the animal was particularly favourable, for he succeeded in exposing the semilunar ganglion and its plexuses with a very trifling loss of blood, not, I believe, four ounces. The heart was found to pulsate sixteen times in ten seconds, as ascertained by the pulsation of the arteries in the neighbourhood of the ganglion. The ganglion and its plexuses were then irritated by the point of the scalpel, and at length cut across in various directions; but although the beats of the heart were repeatedly counted during these operations, they continued uniformly of the same frequency. Spirit of wine was then applied to the wounded ganglion and plexuses, but without the least change in the beats of the heart. A strong infusion of tobacco in water was now applied, but with the same result, the beatings of the heart being still sixteen in ten seconds; nor could any variation in the force of the beats be observed in any part of the experiment.

It appears from this experiment that we cannot influence the organs supplied by the ganglionic nerves by causes affecting the ganglions and plexuses, independently of the brain and spinal marrow; and the inferences from this and the preceding facts are unavoidable, that the former organs make only a part of the channel through which the influence of the latter is conveyed,

[1] It will afterwards appear on what principle this can be done.

and that the peculiar office of the ganglions and plexuses is to combine the influence of the nerves which terminate and are blended in them, and send off nerves endowed with their combined influence, in consequence of which the parts which receive the nerves proceeding from them, become subject to every part of the brain and spinal marrow.

Such being the case with respect to the ganglions and plexuses, it is not likely that we shall find the nerves themselves, whether ganglionic or cerebral, capable of supplying any part of the influence they convey; but that nothing may be taken for granted, this also is a point which must be determined by an appeal to facts.

It is to be recollected that here, as in other cases, the onus probandi rests with the asserter. This would still be the case although his position were less improbable than that, while there is an evident and acknowledged source of nervous influence, and that adequate to the production of the phenomena, another source of it should exist, and that in organs which evidently perform a function of so different a nature. Those who maintain such an opinion must adduce the proofs of it. Let us inquire to what they amount.

While the connexion of the nerves with the brain and spinal marrow exists, the nerves are capable of exciting the muscles both of voluntary and involuntary motion, causing the evolution of caloric which supports animal temperature, forming the secreted fluids from the blood, and supporting the processes of assimilation by which the structure of the various organs is maintained; but as soon as this connexion is intercepted, all these functions begin to fail, and soon cease, nor do we possess a single fact to prove that there are any means in the nerve itself of maintaining or renewing any of them.[1] By mechanical impulse the power which remains in a separated nerve of the cerebral class, for even this is not the case with respect to the gan-

[1] See the preceding parts of this volume.

glionic nerves,[1] may be directed to its extremities and made evident by the excitement of the muscle in which it terminates; but independently of such an impulse, we have no means of exciting a nerve separated from the brain and spinal marrow, even during the short time it retains the influence it has received from those organs.[2]

The very circumstance of the nerves being the means of conveying the influence of the brain and spinal marrow, affords a presumption that they are not themselves the source of a similar influence. The former is evidently their peculiar function, and it is so improbable that they should perform another of so different a nature, that it would require the most unequivocal proof of such a fact to induce its belief. The power of the nerves is not only, as far as we see, derived from the brain and spinal marrow, and soon ceases and cannot be renewed when they are separated from these organs, but is, as I have already had occasion to observe, at all times proportioned to the degree of excitement in them; nor can an instance be adduced in which a cause of increased nervous power makes its impression on the nerves themselves. For its degree as well as existence, then, the power of the latter seems wholly dependent on the former organs; and this observation applies as strictly to the ganglionic as to the cerebral nerves. The brain and spinal marrow, therefore, possess all the characteristics of the active, the nerves of the passive parts of the system.

It may appear at first sight, that the phenomena of what has been termed the sympathy of nerves, oppose the preceding views. On a careful review of these phenomena, however, we

[1] It is true that the heart has been excited by galvanism through the medium of its nerves, but they may here act merely as conductors.

[2] In the living animal, a nerve cut off from direct communication with the brain and spinal marrow, but otherwise uninjured, will, as Sir Benjamin Brodie has shown, long retain this power, as we should *à priori* have expected. It retains its healthy structure, and its communications with other nerves.

shall find, in the last part of the present volume, they afford them additional support. They are all such as depend on changes in the central parts of the nervous system, and in no degree on any influence of the nerves on each other in their progress.

I take this opportunity of directing the reader's attention to a law of sympathy which, as far as I know, has been overlooked, but which has an extensive influence in determining its effects, and consequently the phenomena and progress of disease. We shall find that the functions of the animal body may be divided into two classes, those of the sensitive and those of the vital system. Now, the individual organs do not sympathise equally with the organs of both these sets of functions. Some sympathise most powerfully with those of the sentient and some with those of the vital functions. If we except the brain, whose extensive sympathies are necessary consequences of its functions, the stomach and liver are the organs of most extensive sympathies. Of these organs the former sympathises most with the sentient, the latter with the vital organs. Hence it is that affections of the stomach are more immediately felt in every part of the frame, while those of the liver have a greater influence on the course of disease. The liver being an organ of dull sensation, the share it has in influencing the course of many diseases has often been overlooked, few sensations being referred to the region of this organ, and thus the success of our plans of treatment greatly abridged. But this subject will be resumed at much greater length in the last part of this volume.

An opinion respecting the function of the nerves has been maintained, and lately by a writer of great respectability,[1] which deserves to be considered, because it claims the support of experiment, and, if well founded, must essentially affect our opinion of the nature of the nervous influence.

[1] Dr. William Charles Henry's Critical and Experimental Inquiry into the Relations subsisting between Nerve and Muscle, in the 110th number of the Edinburgh Medical and Surgical Journal.

Dr. Henry appears to admit the independence of the muscular power, but thinks he has rendered it more than probable that the nervous influence, instead of being only one of many agents, is the only one capable of exciting the muscular fibre; and consequently that all others act through it, so that they are not in fact stimulants to that fibre, but to the nerves alone through which they influence it.

It is true that as mechanical impulse affecting a nerve of voluntary motion is capable, after its separation from the brain and spinal marrow, of exciting, through it, the muscle in which it terminates, and we cannot be assured that we have separated from the muscular fibre the whole of the nerves with which it is so intimately blended; if we were in possession of no other facts on the subject, we should be led to the inference that the excitability of the muscular fibre can only be influenced through its nerves; but when, instead of a mechanical, we employ a chemical agent, we find the result very different. We attempt in vain to influence the muscle through the nerve which terminates in it by the most powerful agent of this description, the operation of which is confined to the nerve itself; yet such an agent, when applied to the muscular fibre, excites it as readily as the mechanical agent, which is supposed only to affect it through the nerve.

Even if the power of the chemical agent be gradually increased until the structure of the part of the nerve to which it is applied is destroyed, not only the muscle in which it terminates, but even the other parts of the nerve itself, remain wholly unaffected. The nerve has not even the power of communicating the change to its adjoining parts. This is shown by the experiments of Fontana, and confirmed by those of Dr. Henry related in the paper just referred to.

What reason then have we for supposing, when a chemical agent applied to the muscular fibre excites it, that it operates through the nerves which still adhere to it? Such an inference implies that nerves in their progress wholly change their

nature—a supposition for which there is not only not a shadow of proof, but against which the most convincing proofs, which analogy can supply, present themselves. Besides, it appears from many experiments, that muscles remain for hours obedient both to mechanical and chemical stimulants after all the acknowledged functions of the nervous influence have ceased in all the organs to which they belong.

Dr. Henry indeed adduces in favour of the opinion of agents affecting the muscles only through the intervention of the nerves, a fact which is well ascertained, and which I amongst others have frequently witnessed, that the excitability of distant muscles may be impaired by chemical agents applied to the sentient extremities of the nerves; but here the brain and spinal marrow intervene between the nerves to which the agent is applied and the muscles influenced. The effect of the agent is communicated through the nerves to these organs, and the debility of the muscles is the effect of the morbid impression made on them.

For the same reason a similar effect is produced on the muscles by what surgeons call concussion of the brain. A strong solution of opium or tobacco thrown into the cavity of the abdomen, or suddenly applied to any other extensive and highly sensible surface, has, more or less, the same effect on distant muscles as a blow on the head. It affects them in consequence of the brain and spinal marrow being influenced, and probably, where the cause is most powerful, even to the derangement of their finer mechanism, by the impression made on the sentient extremities of the nerves; so that we have here only an instance of a well-known fact, that certain affections of the brain and spinal marrow are capable of impairing the excitability of the muscles through the medium of the nerves, the only medium of course through which they can operate.[1]

How can we suppose, it has been said in support of the same opinion, that a muscle covered by a membrane of condensed cellular substance, or in other instances by the more complicated

[1] Philosophical Transactions for 1815, and Experimental Inquiry.

serous or mucous membrane, should be affected by a chemical agent applied to the opposite surface of such membranes, if not through the medium of its nerves?

If the agent does not pass through the membrane, and is thus immediately applied to the fibres of the muscle, by what other means can the effect be produced? We have just seen that the nerves are incapable of communicating the impression except where the brain and spinal marrow intervene, and then the effect is general, not local. Besides, those who adopt this explanation forget that in many instances at least, I believe in all, the agent, independently of transudation through membranes, is as little in contact with the nerves as with the muscular fibres. The cuticle possesses neither nerves nor vessels, yet, through it, chemical agents influence the muscles; and are not such agents actually received through the cuticle, and conveyed into the mass of blood?

It seems to be a general law of the animal system, that all membranes are pervaded by certain chemical agents. The air comes not in contact with the blood unless it be through the membranes of the lungs ; yet who doubts that the changes effected in this organ are the consequence of transmission of an agent to or from the blood?

When a more stimulating blood produces a more powerful action of the heart and blood-vessels, can it arise from any other cause than the transmission of the more stimulating agent through the fine membrane which lines these cavities? Without such transmission it is no more in immediate contact with the nerves than with the muscular fibres; and if it were, we know that the impression it makes could not be conveyed through them. The membrane which lines the internal is more delicate than that which lines the external surface of the hollow muscles, and, as we might *à priori* expect, the agent pervades the former more readily than the latter. Hence it seems to be that a strong solution of opium thrown into the cavities of the heart, intestines, &c., immediately destroys their power; while applied to their

external surface it makes no, or comparatively little, impression on it, the final cause of which it is not difficult to perceive. It is necessary that the muscular fibres of those cavities should be exposed to the stimulus of their contents, but their external surface only requires to be supported by the firmness of its membranes.

Must not the chemical agents which influence the nose, the mouth, the fauces, the bowels, the bladder, &c., pervade the fine membranes which line them, before they can act either on their nerves or vessels?

It would appear that there are but two modes of impairing the excitability of the muscles, independently of the immediate application of the agent to the muscular fibre itself; the one I have just had occasion to mention, the effect of powerful agents on the brain and spinal marrow; the other, the excessive excitement of their own function, too powerful and long-continued contractions.

The poisons which impair their excitability affect them only in one or both of these ways, except as far as they act by their immediate application to them. Opium, which was employed by Dr. Henry in the experiments just referred to, acts in all the three ways. But the exhaustion produced in the muscles of voluntary motion when an animal is killed by opium, although in part arising directly from its effects on the brain and spinal marrow, particularly when it has been suddenly applied to a very sensible and extensive surface, is, under other circumstances, chiefly the consequence of the violent spasms excited in them.

The spasms produced by an over-dose of opium always assume the form of opisthotonos, but they are still more subject to remissions than in the disease properly so called; and, as in that disease, they are, during the remissions, often readily renewed by the slightest causes.[1] Even the touch of the finger,

[2] See the account of the Experiments relating to the modus operandi of opium and tobacco, in my Treatise on Febrile Diseases.

although the sensibility, as far as feeling is concerned, is wholly destroyed, is sufficient for this purpose; so that in making the experiment the spasms may be rendered more or less frequent according to the circumstances in which the animal is placed. On examining the state of the muscles of voluntary motion after death, as I have repeatedly done, in animals killed by opium, one being left undisturbed, while in another a constant succession of spasms had been maintained, their excitability in the former I have found little, in the latter greatly, impaired. All will admit that the general spasms here arise from the state induced on the brain and spinal marrow by the opium, and not from any particular change in the nerves; any cause exciting by its action on the former organs, or in any other way, the same contractions, would, we know, produce the same exhaustion.

On reviewing all that has been said, it appears that we have no reason to suppose that the nervous influence excites the muscles on any other principle than that on which all other stimulants operate, which it also resembles in the circumstance of its sudden and excessive application acting as a sedative. I was at much pains, in the last edition of this Inquiry, to point out that all agents capable of influencing either the nervous or muscular system, and whether they make their first impression on the mind or body, act either as stimulant or sedative according to the degree in which they are applied, the stimulant effect always arising from the less, the sedative from the greater, application of them; and different agents being better fitted to produce the one or other of these effects.

Thus with respect to the nervous influence, the more powerful, within certain limits, the action of the agent on the brain and spinal marrow, the greater is the stimulant effect on both classes of muscles; but if it be extreme, as when a severe blow is inflicted on the head, instead of exciting the muscles, it directly impairs their power; in the same way that they are more or less, according to its degree, excited by a moderate application of

electricity; but deprived of all power by its excessive application.

The reader will perceive, as we proceed, how extensively this law operates in the phenomena of disease, and particularly its influence in producing, in many cases, the more immediate cause of death; and from what is said in my Treatise on the Effects of Minute Doses of Mercury, how much the beneficial effects of this and other powerful medicines depend on keeping it in view. It appears, from what is said in that Treatise, that it is to the circumstance of increasing the dose of mercury beyond the limits within which it acts as a moderate stimulant, that all its bad effects are to be ascribed. Within these limits it increases instead of impairing the powers of the system, to which, and the circumstance of our being thus enabled to repeat the dose at short intervals, the effects of this mode of employing it, in gradually recalling the vigour of the debilitated organs, are to be ascribed.

CHAPTER VII.

On the Principle on which the Action of the Vessels of Secretion and Assimilation depends, and the Relation which they bear to the Nervous System.

It not only appears from the experiments which have been laid before the reader, that the power of the heart and vessels of circulation is independent of the nervous system, but that that of the muscles of voluntary motion is so likewise; and that these, like the former, are only subjected to this system in the same way in which they are subjected to every other agent that is capable of exciting them. Thus we find, that all the moving powers of the animal body, as far as we have hitherto traced them, are independent of the nervous system; but that this sys-

tem is equally capable of affecting them, although in different ways, whether they are subject to the influence of the will or not. Is the power of secretion also independent of, though influenced by, the nervous system?

I was soon convinced, that although the powers of circulation are independent of the nervous system, those of secretion are very far from being so. M. le Gallois, in the treatise so frequently alluded to, enumerates many physiologists who divided the eighth pair of nerves; and he gives a minute account of the consequences of their division, particularly of those which he himself observed in rabbits and guinea-pigs. The chief are, oppressed breathing, and loss of power in the stomach. Sir Benjamin Brodie also gives an account of experiments, in which he divided the eighth pair of nerves in dogs, in a paper published in the *Philosophical Transactions* for 1814. But the animals always died of oppressed breathing before he could judge of the effect on the stomach. He proved, however, by another set of experiments, that arsenic introduced into the system after the division of the eighth pair of nerves, does not produce the copious secretion from the stomach and intestines which it is found to do under ordinary circumstances. He found a similar result when he divided the stomachic nerves immediately above the cardiac orifice of the stomach.

The lungs are affected differently according to the part at which the nerve is divided. If in the rabbit it be divided before the inferior laryngal nerve is sent off, or this nerve itself be divided, great difficulty of breathing, with a croaking noise, follows; arising, as M. le Gallois has shown, [1] from the opening of the glottis becoming much narrowed as soon as the nerve is divided. If

[1] M. le Gallois says, that the difficulty of breathing comes on from dividing the recurrent nerves; but Dr. Hastings, who frequently performed the experiment, always found that there was little or no dyspnœa induced in the rabbit by dividing the recurrent nerves. It was when he divided the laryngal nerves that the sudden dyspnœa, mentioned by M. le Gallois, took place.

the eighth pair of nerves are divided below the place at which they send off this nerve, there is little or no dyspnœa for some time. Dr. Hastings, who was so good as to perform the part of operator, and watched the progress of the following experiments with great care, observed, that when the eighth pair of nerves were divided below the inferior laryngal nerve, the dyspnœa, although it often came on sooner, was greatly increased by the attempts to vomit, which generally happened almost immediately after eating; but which, if the animal was not allowed to eat, frequently did not occur for an hour and a half or two hours after the division of the nerves. The dyspnœa increases, and the animal seems at last to die of it. The lungs are found after death considerably altered in their structure, appearing of a more compact texture than healthy lungs, and sinking in water. They are clogged with a frothy fluid, which fills the air-tubes and cells, and covered with patches of a dark red colour, often of great extent, which give the appearance of blood having escaped into the cellular substance.

As M. le Gallois' account of the effects of the division of these nerves on the stomach does not altogether agree with that of the authors he quotes, and is also in other respects contradictory, Dr. Hastings, at my request, repeated the experiment on rabbits, and removed part of the nerve. The author had thus an opportunity of ascertaining, that even during the space of more than twenty hours, although the stomach was full of food, (parsley,) no change on it had been effected. It continued in the same state as when it left the mouth, simply divided by mastication, preserving perfectly both its appearance and its smell. It was impossible to distinguish it from parsley chopped small with a knife.

It occurred to me that the pain and irritation occasioned by the operation might in these experiments have induced such a degree of disease as to destroy the powers of digestion, independently of any specific effect on the stomach. I therefore re-

quested Dr. Hastings to perform the experiment in the following manner:—

Exp. 53. Two rabbits, of about the same age, were fed in the same way. In both, the eighth pair of nerves were brought into view. In the one rabbit a part of each nerve was removed; in the other, after being raised on a probe, they were replaced without injuring them. Both rabbits were allowed to eat as much parsley as they chose after the operation. When that, in which part of the nerves was removed, died, which did not happen for more than twenty hours after the operation, the other was killed. In the former the food was found wholly undigested; in the latter the digestive process had gone on as usual, and the food was found in the same state as in a healthy rabbit.

The stomach is generally distended to a greater size than usual, when the eighth pair of nerves has been divided, the cause of which will hereafter appear. This happened in the present case. It is remarkable, that the œsophagus, also, is found to contain food, and is often very much distended. From these circumstances, and from an experiment of M. le Gallois, in which one only of the eighth pair of nerves being divided in a guinea-pig, the animal survived several days, and the stomach became excessively distended with undigested food, it occurred to me that the sensation by which an animal judges when he has received enough of food, being destroyed by the division of the nerves, the animals had perhaps occasioned over-distension of the stomach, and thus destroyed the power of digestion, for they often ate a great deal after the operation. I therefore requested Dr. Hastings to repeat the experiment, allowing the animal to eat as much as he chose before the operation, but none after it.

Exp. 54. This he did, but the result was the same. The food with which the rabbit had filled its stomach just before the division of the nerves, remained wholly unchanged; and it was remarkable that the œsophagus was just as much distended with the food as when the animal had eaten after the operation. This

arises from the fruitless efforts to vomit, which always come on in an hour or two after the division of the nerves.[1] It deserves notice, that although part of the eighth pair of nerves has been removed, the food is found covered with apparently the same semifluid which is found covering the food in a healthy stomach.

It will be admitted, I think, that these experiments leave no room to doubt that the office of the stomach is suspended by removing part of the eighth pair of nerves. A similar observation applies to the lungs. When the whole of the influence conveyed by these nerves is thus prevented, and the animal survives eighteen or twenty hours, the contents of the stomach are found not only undigested but quite dry, showing that all secretion of gastric juice had for some time ceased. When the nerves are merely divided the digestion is deranged, not arrested, but the proportion of fluid found in the stomach is then greater than usual.

In the animal in which the eighth pair of nerves was merely raised on the probe, the lungs continued perfectly to perform their office, and were found of a healthy appearance after death. In all the instances in which a part of the nerves was removed, great dyspnœa soon came on, the structure of the lungs was evidently altered, the air-cells and tubes were found clogged with frothy mucus, and the surface of the lungs marked with dark-coloured patches.

It appears then, that the extreme parts of the sanguiferous and nervous systems are connected in a way very different from that in which these systems are connected in other parts. The heart and vessels of circulation can perform their functions after the nervous influence is withdrawn. The power of secretion immediately ceases on the interruption of this influence. We must

[1] I have already mentioned that Dr. Hastings observed the dyspnœa greatly increased by the fruitless attempts to vomit. As the passage to the stomach in the rabbit lies contiguous to the yielding part of the windpipe, the distension of the former cannot fail to lessen the capacity of the latter.

suppose, therefore, either that the influence of the nervous system bestows on the extreme vessels the power of separating and re-combining the elementary parts of the blood, or that the vessels only convey the fluids to be operated upon by this influence.

Experiments, related in the second chapter of the present part of this Inquiry, show that the most minute vessels which can be seen by a microscope in the web of a frog's foot, are independent of the nervous system. The motion of the blood is as rapid, and the circulation in the foot presents precisely the same appearance after, as before the destruction of that system. It is hardly consistent with these experiments to suppose that any part of the sanguiferous derives its power from the nervous system. If the power of the vessels of secretion had been lost by the destruction of the latter, this must have occasioned some change in the distribution and motion of the blood in the web. The conclusion from these experiments is strengthened by other facts. In those experiments in which the power of secretion was impaired by withdrawing the nervous influence, this appeared to be in no degree the effect of any deficient supply of fluids. When it was more partially withdrawn, the fluids of the part were even more copious than usual. The slightest failure of nervous influence seemed to cause a degree of irritation that stimulated the vessels to pour out a greater than due proportion of fluid, a greater failure, to withdraw from them all stimulus, so that they poured out none. The fault seemed to be, that a due change on them had not been effected. We have no reason to believe, we have seen, that the vessels possess any other than a muscular power, if we except the mere power of elasticity. Now it has been proved, by direct experiment, that the muscular power throughout the whole animal, namely, in the muscles of voluntary motion, the heart and the vessels of circulation, is independent of the nervous system. Can we suppose that the vessels of secretion, which are only a continuation of those of circulation, all at once assume a different nature? Or, is it at all consistent with our

knowledge of the phenomena of chemistry, to conceive that, by any influence, the muscular and elastic powers can be enabled to separate and re-combine the elementary parts of the blood?

The first of the above positions, therefore, may, as far as I can judge, be regarded as set aside. This admitted, it seems a necessary result of the preceding experiments and observations, that in the function of secretion the vessels only convey the fluids to be operated upon by the nervous power. The reader will soon be presented with other facts which tend to confirm this inference.

Thus it appears that the vessels of secretion, like those of circulation, are independent of the nervous system; secretion failing when the influence of this system is withdrawn, not because the vessels of secretion fail to perform their office, but because the necessary changes on the fluids which they supply no longer take place.

We know that the nervous power occasionally influences the vessels of secretion, as we have seen it does those of circulation, because affections of the mind and other causes affecting the nervous system frequently influence the flow of fluids to secreting surfaces. The vessels of secretion, therefore, thus far obey the same laws as those of circulation; they are independent of, but influenced by, the nervous power. Are they influenced by certain parts of the brain alone, or, like the heart, by every part of the brain and spinal marrow? To save repetition, this question will be considered, with another intimately connected with it, in the second section of the tenth chapter.

It is not to be overlooked, that the vessels convey the fluids, to be operated upon by the extreme parts of the nervous system, in a peculiar way. By the lessening capacities of the capillary vessels, the blood is divided as by a fine strainer, some of its parts being too gross to enter the smaller vessels. How far the blood may thus be subdivided, and how far it may be differently divided in different organs, we cannot tell. As this structure of the ves-

sels is uniform, we have reason to believe that its effect on the blood is necessary, to prepare this fluid for the due action of the nervous power.

CHAPTER VIII.

On the Effects on the Lungs of removing a portion of each of the eighth pair of Nerves.

However much the secreting surface of the stomach may be deranged by the means just mentioned, its appearance, owing, we have reason to believe, to the extreme minuteness of its structure, is the same, or nearly so, as when the nerves have been left undisturbed; and with the exception of occasional efforts to vomit, no symptom shows itself after the division of the nerves indicating the derangement of function which has taken place. Both in the symptoms and appearances after death, the derangement, occasioned in the lungs by the interruption of a considerable portion of their nervous influence, is much more evident; the function as well as the structure of the lungs being, from their nature, more readily made the subjects of observation.

Soon after the operation the animal begins to breathe with difficulty, and this symptom gradually increases, and is at length evidently the cause of death. On inspecting the lungs after death, the air-tubes and cells, as far as they can still be traced, are found to contain a viscid fluid; and in considerable portions of the lungs, generally more or less according to the time the animal has survived the operation, every trace of both tubes and cells appears to be obliterated, the lungs both in colour and consistence assuming much of the appearance of the liver, and these portions of lungs sink in water; and although examined with the greatest care, and the aid of a powerful magnifying glass, both by Mr. Cutler, who

was so kind as to give me his assistance, and myself, we could not perceive in them the least remains of the structure peculiar to this viscus.

I wished, however, to ascertain, by means less fallacious than the sight, whether the change of structure in the parts most affected, be such as to cause the total obliteration of their cavities, and Mr. Cutler, at my request, was so obliging as to make the following experiments, the account of which I shall give in his own words.

Exp. 55. "If you cut out a portion of each of the eighth pair of nerves in the neck of a rabbit, it seldom dies within eight hours, and rarely survives more than twenty-four hours.

"On examination after death, the lungs are found, in many parts, covered with dark red patches.

"To ascertain the mischief done to the substance of the lungs, I endeavoured to fill them with mercury by the trachea, but from the delicate structure of the air-cells a rupture took place, and the mercury escaped.

"I then endeavoured to inject the air-cells through the trachea with the finest vermilion injection. In the healthy lungs the attempt was invariably successful, making the whole of a bright scarlet colour, and, on cutting into them, every part was found to be uniformly filled with the injection.

"After injecting the diseased lungs, the dark red patches remained on their surface; other parts of the lungs were of a bright red colour: some parts were partially injected, and other parts retained their natural appearance.

"This was explained on dissection. Those parts of the lungs which were completely injected had not suffered from disease, other parts had suffered sufficiently partially to obstruct the injection, while some parts were so completely hepatised, that not a particle of injection could enter them, or the parts beyond them, which were not equally diseased.

"Those portions of the lungs which were completely injected, sunk in water, from the weight of the injection.

"The hepatised portions, from their diseased state, sunk also, whilst the portions beyond them, having their natural appearance, floated."

It appears from these statements, that the effect of dividing the nerves of a vital organ, and separating the divided ends, is not merely that of deranging its secreting power, but all its assimilating powers, namely, all those powers on which its healthy structure depends.

CHAPTER IX.

On the Principle on which the Action of the Alimentary Canal depends.

In order to ascertain how far the peristaltic motion of the intestines is independent of the brain and spinal marrow, the following experiments were made.

Exp. 56. A rabbit was killed in the usual way. The whole of the spinal marrow was then destroyed by a small hot wire. On opening the abdomen, we found the peristaltic motion of the stomach and small and great intestines quite as strong as when the nervous system is entire, as we ascertained by exposing the abdominal viscera of other newly-dead rabbits. This motion is as strong in the newly-dead as in the living animal.

Exp. 57. The spinal marrow was wholly removed in another newly-dead rabbit, without at all affecting the motion of the stomach and intestines. The removal of the brain produced as little effect upon it as that of the spinal marrow. When both were removed at the same time, it remained unaffected. It continues till the parts become cold, so that when the intestines exposed to the air have lost their power, that of those beneath still remains; even the removal of the intestines from the body does

not at all impair their pereristaltic motion, and only causes it sooner to cease from the exposure to cold and irritation.

From these experiments, compared with those made with a view to ascertain the principle on which the power of the muscular fibre depends, it appears that the muscular power of the stomach and intestines, like that of the heart and blood-vessels, resides in themselves, and is wholly independent of any influence derived from the nervous system.

CHAPTER X.

On the Relation which the Alimentary Canal bears to the Nervous System.

The alimentary canal is of such importance in the animal economy, that it is of the first consequence, in tracing the laws of the vital functions, to ascertain the principle on which its action depends, and the relation which subsists between it and the nervous system; the former of these points I have endeavoured to ascertain by the experiments related in the preceding chapter; the latter I am now to consider.

Exp. 58. I endeavoured, in the newly-dead animal, to ascertain how far the motion of this canal is influenced by stimulants applied to the brain and spinal marrow; but, from its nature, it is in every way so irregular, that no certain result could be obtained. It often appeared that spirit of wine applied to the brain and spinal marrow increased it.

The admission of air into the cavity of the abdomen, in the newly-dead animal, throws the bowels into strong spasmodic action, which alone would obscure any effect that can be supposed to arise from stimulating the brain. The abdomen of the newly-dead animal was therefore opened under tepid water, but this was found to excite even stronger spasms than the air had done.

The effects of the passions on the alimentary canal, however, leave no room to doubt that it is capable of being stimulated through the nervous system. It remains to be ascertained whether it is subject only to certain parts of the brain, or, like the heart, to every part of that organ and of the spinal marrow.

It is evident from the circumstances just mentioned, that it is impossible to answer this question respecting the alimentary canal as respecting the heart, by agents applied to different parts of the brain and spinal marrow. Before the experiments which were resorted to for this purpose are related, it will be proper to direct the reader's attention to the process of digestion in the animal on which these experiments were made, which will place in a clearer point of view both their results, and those of some experiments already laid before him.

CHAPTER XI.

On the Process of Digestion.

On the functions of the stomach all other functions of the animal body may be said to depend, as their various organs derive from it that supply, without which they can exist only for a very short time. In another point of view we find the stomach equally important. There is no other organ whose diseases are at once more frequent and varied, or which partakes more of the diseases of other parts, or of the whole system.

It is not, however, my intention to enter fully into that part of the process of digestion which is performed in the stomach. The experiments of Spalanzani and others sufficiently prove that the change which the food undergoes in this organ is effected by the solvent power of a fluid secreted by it. I shall confine myself to such circumstances attending this change as serve to elucidate the results of the experiments which have been or are about to be laid before the reader.

I have inspected after death, under various circumstances, and at different periods after taking food, the stomachs of about a hundred and thirty rabbits, which has enabled me not only to ascertain some points that will place the result of many of my experiments in a clearer light, but to observe the process of digestion more particularly than has hitherto been done.

The experiments on this part of the subject were so frequently repeated, that it would be tedious and unprofitable to give an account of each experiment. Under the head Experiment, therefore, the result of all the experiments on each particular part of the subject will be given. Mr. Sheppard was so good as to assist me in these experiments.

Exp. 59. The first thing which strikes the eye on inspecting the stomachs of rabbits which have lately eaten, is, that the new is never mixed with the old food. The former is always found in the centre, surrounded on all sides by the old food, except that on the upper part, between the new food and the smaller curvature of the stomach,[1] there is sometimes little or no old food. If, as was ascertained by more than twenty trials, the old and the new food are of different kinds, and the animal is killed after taking the latter, unless a great length of time has elapsed after taking it, the line of separation is perfectly evident, so that all the old may be removed without disturbing the new food. For this purpose we fed rabbits on oats, and, after making them fast for sixteen or seventeen hours, allowed them to eat as much cabbage as they chose, and killed them at different periods, from one to eight hours after they had eaten it.

On opening the stomachs of rabbits three or four weeks old, who both sucked and ate green food, the curdled milk was always found unmixed with the green food. Before the stomach was

[1] The stomach is like a bag suspended by its extremities, which, when filled, bulges out on the lower part. Thus the distance of the extremities measured by the lower is greater than that measured by the upper part. Hence the lower part of the stomach is by anatomists called the greater, and the upper part the smaller curvature of the stomach.

opened, it was evident, from its transparency, where the green food and where the milk lay. The rabbits used in this and all the experiments which are related in this section, were killed in the way usual for the use of the table.

Exp. 60. If the old and the new food be of the same kind, and the animal be allowed to live for a considerable time after taking the latter, the gastric juice, passing from the old to the new food, and changing as it pervades it, renders the line of separation indistinct, so that on a cursory view we should suppose the old and new food mixed together; but towards the small curvature of the stomach, and still more towards the centre of the new food, we find it, unless it has been very long in the stomach, comparatively fresh and undisturbed. All around, the nearer the food lies to the surface of the stomach, the more it is digested. This is true even with regard to the food in the small curvature, compared with that nearer the centre, and the food which touches the surface of the stomach is more digested than any other found in the same part of the stomach. But unless the animal has not eaten for a great length of time, that in contact with the surface of the stomach is in very different stages of digestion in different parts of this organ. It is least digested in the small curvature, more in the large end, and still more in the middle of the great curvature.

These observations apply to the left portion of the stomach which first receives the food, and is called the cardiac portion, from the orifice by which the food enters, termed by anatomists the cardia. Sir Everard Home, in his work on Comparative Anatomy, has shown that the stomach is divided into two portions, in such a way, that the length of the first is to that of the other nearly as two to one. The latter is called the pyloric portion, from the name of the orifice by which the food leaves the stomach. The line of division may generally be seen in some animals after death. He says it is more evident while digestion is going on. I have sometimes observed it very distinctly after death in the stomach of the rabbit, and have then found the food in the two cavities divided by an evident line of separation as described by

this author. The two portions of the stomach form an angle with each other, which is well expressed by the plates in Sir Everard Home's work, and appears to me to cause the line of division just mentioned.

Exp. 61. The food in the pyloric portion of the stomach of the rabbit, is always found in a state very different from that just described. It is more equally digested, the central parts differing less in this respect from those which lie next the surface of the stomach; it is evident, however, that all the change effected in the stomach is not completed when the food enters this portion of it, because we find it the more digested the nearer it approaches to the pylorus, where, being ready to pass into the intestine, it has undergone all that part of digestion which is performed in the stomach.

One of the most remarkable differences between the state of the food in the cardiac and pyloric portions of the stomach, is, that in the latter it is comparatively compact and dry, in the former mixed with a large portion of fluid, particularly when digestion is pretty far advanced, and time consequently has been given for a considerable secretion from the stomach. In the rabbit, indeed, which is fed only with solid food, in the early stage of digestion it is nearly as free from liquid in the cardiac as in the pyloric portion of the stomach. When digestion is very far advanced, the whole contents of the former are often reduced to the state of a semi-fluid. But even then the contents of the pyloric portion, particularly those parts which are near the pylorus, are comparatively compact and dry. In rabbits so young as to live wholly on milk, the curdled milk is considerably softer and moister in the cardiac than in the pyloric end of the stomach.

Exp. 62. Although the food is in the most digested state in the pyloric, it appears from several circumstances that the change is chiefly effected in the cardiac end, which from its greater capacity is also called the great end of the stomach. The food found in the pyloric end, we have just seen, is comparatively dry,

while that found in the great end, if digestion is much advanced, is mixed copiously with the juices of the stomach, and there is a more evident difference in the state of the food before it comes into this part, and when it is about to leave it, than in any other part of the stomach. I shall presently have occasion to mention a fact ascertained by Mr. Hunter, which seems to confirm this opinion. Dr. Hastings, on examining the stomach of a woman who had died under his care, found it everywhere in a state of ulceration, except in the great end, where it was healthy. The stomach had performed its functions to the last, and the contents of the bowels proved that the food had been properly digested.

It appears that in proportion as the food is digested, it is moved along the great curvature, where the change in it is rendered more perfect, to the pyloric portion. Thus, the layer of food lying next the surface of the stomach is first digested. In proportion as this undergoes the proper change, it is moved on by the muscular action of the stomach, and that next in turn succeeds to undergo the same change. As the gastric juice pervades the contents of the stomach, though apparently in no other way than by simple juxtaposition, (for the arrangement of the food, above described, was never found disturbed,) the change in each part, which in its turn comes in contact with the stomach, is far advanced before it is in actual contact with it; and, consequently, is soon after this in a proper state to be moved on towards the pyloric end.

Thus a continual motion is going on, that part of the food which lies next the surface of the stomach passing towards the pylorus, and the more central parts approaching the surface. When rabbits have fasted sixteen or eighteen hours, the whole food found in the cardiac portion, which is in small quantity compared to what is found in it even five or six hours after a full meal, seems frequently to be all nearly in the same state with that next its surface, the gastric fluid having pervaded and acted upon the whole, and is consequently apparently fitted to be sent to the pyloric end. Sir Everard Home found that the stomach

of a rabbit never empties itself, containing, even when the animal dies after long fasting, a considerable quantity of food. The first impression on the food is made in the small curvature, because here the new food first touches the surface of the stomach, and the upper part of the new food, which lies contiguous with this part of the stomach, or nearly so, is always found more changed than the more central parts of it.

In the large end of the stomach I frequently found the small round masses or balls, about the size of the largest kind of shot, mentioned by Sir Everard Home. These balls are very constantly found in the great end of the stomach of rabbits, especially when fed on green food, never in any other part of it. They are often very numerous, sometimes forming the whole contents of that part of the stomach. They cannot be fewer, in many cases, than from two to four hundred. At other times they are much less numerous, and mixed with food of the same consistence with that of which they are composed. It is difficult at first view to account for their appearance. The opinion of Sir Everard Home, that they are produced by the rabbit occasionally ruminating, is opposed by several circumstances; the apparent impossibility of rumination producing the effect, the frequency of their appearance, their sometimes forming one half or more of the contents of the stomach, their being always found at a considerable distance from the opening by which the food enters the stomach, unless their number is so great as to fill the greater part of the stomach, food much less digested than that composing them generally lying between them and this opening, their being always too soft to retain their shape in the act of deglutition, and no appearance of this kind being found in ruminating animals.

It was long before I could form even a probable conjecture respecting the formation of these balls. I have now, from inspecting many stomachs containing them, very little doubt of the cause to which they are to be ascribed. When the stomach of the rabbit is laid open, the great end is found corrugated, the

rugæ giving it a honeycomb appearance. These rugæ disappear when it is stretched, and as soon as the stretching power is withdrawn, again appear, the surface of the rest of the stomach being comparatively even. The balls seem to be formed in the hollows of these rugæ, which are about the same size with the balls. It would appear that the food, by the action of this part of the stomach, is rolled up into these masses after it has undergone that part of the digestive process which takes place in the great end of the stomach, and, consequently, after it has been exposed for a considerable time to the action of the gastric juice; in which form it is sent towards the pyloric end, where the balls are broken down, and the whole again formed into one mass of a firmer consistence than the balls. The formation of these balls appears to be in some degree similar to the process by which, in many animals, the discharge from the bowels is formed into similar though much larger and firmer balls by the intestinal cells. I have observed that when all the food in the great end of the stomach is composed of them, it contains no fluid but that which is mixed up with the food in them. Sometimes no balls are formed. This is comparatively rare. We never found the curdled milk formed into balls, consequently there are none in the stomachs of very young rabbits. With this exception they are frequently, it may be said very generally, found under all circumstances of diet, situation, &c. Sometimes when rabbits had lived precisely in the same way, they were not found in all. They are sometimes found, when the more central parts of the contents of the stomach have undergone little or no change.

Their formation is evidently no essential part of the digestive process, and is probably prevented by food or air occasionally so distending the stomach during that process as to prevent the formation of the rugæ just described. It would appear from several observations, that when the whole of the contents of the great end of the stomach have equally undergone the action of the gastric juice, so that there is no fresher food in the central parts, they are very slowly passed into the pyloric portion. Hence

it seems to arise, that they are often, when the animal has fasted for a considerable time, and thus the stomach, by the diminution of its contents, is allowed to fall into the rugæ, wholly formed into balls; when it has fasted for a great length of time, so much fluid is secreted in the great end of the stomach, that the balls lose their consistency, and the whole runs into a uniform semi-fluid mass.

Exp. 63.—It is in the great end of the stomach where digestion appears to go on so rapidly, that Mr. Hunter found the stomach itself dissolved; and by the most satisfactory arguments showed that this is the effect of the gastric juice after death. His observations on this subject confirm the foregoing view of digestion, for he found part of the stomach digested when the food it contained remained undigested, in the case of a man killed immediately after a full meal. This I have often observed in rabbits, when the animal has been killed immediately after eating, and allowed to lie undisturbed for some time. On opening the abdomen I have found the great end of the stomach soft, eaten through, sometimes wholly consumed, the food being only covered by the membrane which covers the viscera, or lying quite bare for the space of an inch and a half in diameter, and part of the contiguous intestines, under the latter circumstances, also consumed; while the cabbage, which the animal had just taken, lay in the centre of the stomach unchanged, if we except the alteration which has taken place in the external parts of the mass it had formed, in consequence of imbibing gastric juice from the half-digested food in contact with it. The great end of the stomach was sometimes found consumed within an hour and a half after death; this was more frequently the case when the animal had lain dead for many hours. It is not always injured, however long the animal has lain dead. This seems only to take place when there happens to be a greater supply than usual of gastric juice. Thus it was always observed most apt to happen when the animal had eaten voraciously. Why it should take place without the food being digested is evident from what has

been said. Soon after death, the motion of the stomach, which is constantly carrying on towards the pylorus those portions of food in which that part of digestion which is completed in the stomach has taken place, ceases. Thus, the food, which lies next to the surface of the stomach, becoming fully saturated with gastric juice, neutralizes no more; and no new food being presented to it, it necessarily acts on the stomach itself, now deprived of life; and on this account, as Mr. Hunter justly observes, equally subject to its action with other dead animal matter. It is remarkable that the gastric juice of the rabbit, which in its natural state refuses animal food, should so completely digest its own stomach, as not to leave a trace of the parts acted on.[1] I never saw the stomach eaten through except in the large end, an additional proof that it is in this part of the stomach that the change in the food is chiefly effected. In other parts its internal membrane is sometimes injured.

It will be interesting to recur to the effects produced on the stomach by depriving it of a great part of its nervous influence by dividing the eighth pair of nerves, while the foregoing account of the process of digestion is kept in view.

The division of the eighth pair of nerves, which I have so frequently had occasion to mention, is one of the oldest physiological experiments of which we have any account. It was performed by several of the ancients, and has been repeated by a great many physiologists in modern times. Valsalva is among the first who gave any distinct account of its effect on the stomach. He observes that it impedes digestion, and even prevents the food passing from the œsophagus into the stomach. The cause of part of the food being found in the œsophagus, I have had occasion to point out above.[2]

[1] The rabbit when hungry will often eat animal food.

[2] It is said that M. Magendie has divided the eighth pair of nerves immediately above the diaphragm, and found that the stomach is still capable of performing its functions. Of the effects of the division of the eighth pair of nerves at this place, I cannot speak from my own observa-

If the animal be allowed to live for a considerable time after a part of the eighth pair of nerves is removed, the food remaining in the stomach is always found undigested, and nearly in the same state in all parts of the stomach, a circumstance which the author was at first greatly at a loss to explain. This effect was uniform; I never saw it otherwise. Yet we must conceive that at the time the animal last eats, there is some food more or less digested in its stomach, and some gastric juice to act on part of that just received into it. The foregoing statements explain the difficulty. The division of the eighth pair of nerves destroys the formation of the gastric juice; but the animal still living, and the motions of the alimentary canal, as the reader has seen, being independent of the nervous power,[1] the usual motions of the stomach continue, and send onwards into the intestines all the food which is in any considerable degree digested, and, consequently, can so affect the stomach as to excite its natural motions.

Thus it is evident from the foregoing observations, that the undigested food must at length come into contact with it. As soon as this happens, the usual secretions not being supplied to produce the proper change in the food, an unnatural motion is excited; hence the efforts to vomit, which always ensue in about an hour, an hour and a half, or two hours, after the division of the nerves, marking the time when the stomach, having sent towards the pylorus its digested contents, begins to feel the effects of undigested food coming into contact with it. If the animal be allowed to eat after the operation, the efforts to vomit almost

tion, as I have never seen the experiment made. Its effects on the stomach we should expect to be different from those of their division in the neck, because they form various connexions with the great sympathetic nerve before they arrive at this place. By dividing and separating the divided ends of the eighth pair of nerves in the neck, the stomach is deprived of the whole power of these nerves, except what may be received by anastomosis with the great sympathetic nerve previously to its division, which must be trifling.

[1] See Chap. VI.

immediately ensue; the new food, as appears from the above statement, and indeed as is evident from the way in which it enters the stomach, directly coming in contact with some part of the small curvature.

We thus see the cause of the efforts to vomit, which follow the division of the eighth pair of nerves; and why, if the animal be allowed to live for a certain time after the operation, nothing but undigested food is found in the stomach. This effect takes place slowly, because the less the food is digested, the more slowly and with the greater difficulty is it transmitted into the intestines. It also appears, from the same circumstances, why the stomach is generally more distended than usual after the eighth pair of nerves have been divided, particularly if the animal has been allowed to eat after the operation; all the food, not digested by the gastric juice, present before the division of the nerves, remaining in the stomach, and there swelling from the heat and moisture.

It is almost unnecessary to add, after all the explanations which have been given, that I never saw the surface of the stomach in the least injured after a portion of each of the eighth pair of nerves had been removed, however long the animal had been allowed to lie dead.

CHAPTER XII.

On the Effects on the Stomach and Lungs of destroying certain Portions of the Spinal Marrow, compared with those of removing a part of one or both of the Eighth Pair of Nerves.

From the extreme irregularity of the motions of the alimentary canal, we have seen, we cannot ascertain whether it is subject to the influence of the different parts of the brain and spinal marrow in the way in which this has been done respecting the heart. I, therefore, endeavoured to ascertain this point by withdrawing

from the most important part of this canal, the stomach, the influence of different parts of these organs, and observing the effects produced on it.

As the reader has seen the office of the stomach destroyed by removing a part of each of the eighth pair of nerves, he might at first view infer, that it is from the brain alone that the stomach derives its nervous power. But although the process of digestion is destroyed by this means, it does not follow that the stomach may not derive nervous power from other sources, because the loss of any considerable part of its nervous power may destroy its function. Besides, its remaining sensibility, indicated by the efforts to vomit, proves that its nervous power was not wholly withdrawn.

If then this power be not supplied to the stomach by the eighth pair of nerves alone, but also, as we have reason to believe from the evidence of anatomy, by nerves arising from all parts of the spinal marrow, it is evident, that cutting off its supply from any considerable part of this organ, while we leave the eighth pair entire, must affect its power, though perhaps not so much, because the brain, we have reason to believe, constitutes the most important, as well as the largest, part of the nervous system. To ascertain this point, the following experiments were made.

Exp. 64. An opening was made about the middle of the spine, and the lower part of the spinal marrow destroyed by a small wire. The only apparent effect of the operation was the loss of power and feeling in the lower part of the animal. It seemed to be otherwise in health. It may here be observed, that the sufferings of the animal in those experiments, in which portions of the spinal marrow were destroyed, was much less than at first view they appear to be; because as soon as the spinal marrow is divided, the sensibility of all parts below the division is lost. The animal, used in this experiment, was allowed to eat nothing for twelve or fourteen hours after the operation. At the end of this time it ate parsley very readily, and in large

quantity, without any tendency to vomit. It lived twenty-four hours after the operation, and ate parsley from time to time.

On examination after death, the stomach was found distended to a great degree, apparently containing the whole of the parsley which had been eaten after the operation, in an undigested state. The bladder had not been emptied after the operation, and it was so much distended, that it rose above the umbilicus. The lower intestine was also distended, although there had been some discharge from the bowels. The lungs collapsed on opening the thorax, but were slightly congested, that is, obstructed with phlegm.

Exp. 65. In a full-grown rabbit a small wire was introduced into the spine at the fourth lumbar vertebra, by which it was attempted to destroy the spinal marrow as far as the first dorsal vertebra. The hind legs were rendered insensible and motionless. Respiration was a little disordered. In a short time after the operation the animal appeared lively, and ate some parsley. The respiration continued to be slightly affected. Some hours after the operation, Dr. Hastings, who watched the animal, observed it to be very cold, and it shivered, although it was kept in the same temperature with other rabbits, who showed no signs of being cold. The rabbit used in the last experiment also seemed cold, but not in the same degree. The respiration now seemed much disordered, and the animal refused parsley. It was then brought near a fire and wrapped up in flannel. By these means it was soon relieved, the shivering ceased, its eyes looked more lively, and the breathing became more free. It was kept near the fire as long as it lived, and frequently ate parsley. It died twenty-seven hours after the operation.

The stomach was not much distended. The parsley near the cardiac orifice was not at all changed, and that near the pyloric orifice very slightly. The membrane of the windpipe and its branches was more vascular than natural. The cells were slightly loaded with frothy and bloody mucus, and there were the same

red patches in the lungs as after the division of the eighth pair of nerves.

Blood was extravasated in different parts of the spinal marrow, as far as the wire had passed, and the membranes were inflamed. Immediately above the opening, the spinal marrow was quite destroyed for about an inch in length. In other places it did not appear much injured.

Exp. 66. I wished to ascertain the effect of destroying a smaller portion of the spinal marrow than that destroyed in either of the last experiments, and directed Dr. Hastings to perform the following experiment, noting the temperature of the animal at the different periods of it. In a rabbit about two months old, fed on parsley, a small wire was introduced into the spinal canal, at the first lumbar vertebra, and that part of the spinal marrow which lies below this vertebra destroyed. After mentioning the other circumstances of the experiment, I threw together the observations made on the temperature. The animal lost the power and feeling of the lower extremities, but seemed in no other way particularly affected by the destruction of this part of the spinal marrow. It lived thirty-five hours.

On examination after death, the stomach was found no larger than natural, the parsley retained its colour, smell, and fibrous texture, although such a change had taken place in it, as demonstrated a very slight degree of the digestive process. The duodenum, for about an inch below the pylorus, was filled with parsley in the same state. The bladder and rectum were distended, but not so much as in the two last experiments. The lungs were slightly congested.

It is difficult to destroy a large portion of the spinal marrow without immediately killing the animal. It must be done very slowly, and even with this precaution the attempt will not always succeed. Its destruction is attended with no suffering to the animal, if we previously divide it at the upper part of the portion which we wish to destroy. On examination after death, the lum-

bar portion of the spinal marrow was found to be completely destroyed.

The following are the observations on the temperature.

The bulb of Fahrenheit's thermometer introduced into the mouth, and kept there for two minutes previous to the experiment, stood at 98°.

The animal was kept in a warmer temperature than before the destruction of the lumbar portion of the spinal marrow. The temperature was always measured by putting the bulb of the thermometer into the mouth, and keeping it there for two minutes.

Immediately after the operation, therm.	98°
In twelve minutes after it	92°
In half an hour after it	92°
In two hours and a half after it	98°
In five hours and three-quarters after it	98°
In seven hours and a quarter after it .	98°
In nine hours after it	96°
In ten hours after it	95°

The animal during all this time appeared lively, and ate parsley.

In eleven hours after it, therm.	96°
In twelve hours after it	97°

Night coming on, the temperature was not measured again for thirteen hours. In the morning the rabbit appeared lively, and ate readily.

In twenty-four hours after the operation, therm.	88°
In twenty-seven hours after it, therm. .	84°
In twenty-nine hours after it	88°
In thirty hours after it	84°
In thirty-one hours after it	84°
In thirty-three hours after it	80°

The animal still continued to eat. In thirty-four hours after the operation the temperature was 75°. In an hour after this the animal died. This animal did not appear nearly so cold as

that in the preceding experiment, in which a larger and more important part of the spinal marrow was destroyed.

Thus we find the function of the stomach impeded by depriving it of the influence of any considerable part of the spinal marrow, and it seems only more affected by the division of the eighth pair of nerves, in proportion to the greater extent and importance of the brain. Indeed, there is reason to believe that if the whole spinal marrow could be destroyed, without killing the animal, we should find the lungs and stomach quite as much disordered as by the division and separation of the divided ends of both the eighth pair of nerves.

It is remarkable, that the result of the first of these experiments is the same with that which M. le Gallois obtained when he had divided one of the eighth pair of nerves in a guinea-pig. The animal did not vomit, and the stomach was found distended to a great size, apparently containing all the food it had taken after the operation, in an undigested state. This coincidence demonstrates how much the same the effect on the stomach is, whether we deprive it of part of the nervous power which it receives from the brain, or part of that which it receives from the spinal marrow. Dr. Hastings, at my request, made the following experiment.

Exp. 67. Part of one of the eighth pair of nerves was removed in a rabbit. It ate soon after the operation, but did not attempt to vomit till two hours and a half after it, and then dyspnœa came on. The breathing at times was almost free, and the efforts to vomit only occurred at intervals. Neither symptom supervened when it was prevented from eating. It died in forty eight hours after the operation.

The stomach was found after death larger than natural, being distended with air, and containing more food than usual. For the most part the parsley was in the same state as when taken into the stomach, both in appearance and smell. In some places it was slightly changed. There was undigested parsley in the duodenum, to the distance of about an inch from the stomach.

The lower end of the passage to the stomach contained a little parsley. There was none in any other part of it.

The membrane of the windpipe was of a darker colour than natural, its vessels being distended with blood, and there was some frothy mucus in it. The lungs were slightly congested. The membrane of the air-tubes was too vascular, and the air-cells contained some frothy mucus. The lungs collapsed imperfectly on opening the chest. There were some dark-coloured spots on them. All these appearances existed in a much less degree than when both nerves were operated on.

If the reader will take the trouble to compare these appearances with those observed when part of the spinal marrow was destroyed, particularly in Experiment 64, he will see that the division of one of the eighth pair of nerves produces nearly the same effect on the lungs and stomach, as the destruction of part of the spinal marrow.

I wished to see the effect on the stomach and lungs of destroying nearly the whole spinal marrow; but, with all the precautions that could be taken, the animal died almost immediately. It is difficult indeed to prevent immediate death, when as much of it is destroyed as in Experiment 64.

There is another point of no small importance in the animal economy, relating to this part of the subject, which remains to be ascertained. Do the effects observed in the stomach and lungs, when part of the spinal marrow is destroyed, arise from the destruction of that part, that is, from the ceasing of its office; or from the influence of the brain on the spinal marrow being thus limited? It is evident, that if the former opinion be correct, the division of the spinal marrow in the middle, will not produce the same effects as the destruction of the lower half. If the latter be correct, these must produce similar effects.

Exp. 68. The spine was divided in an old rabbit, about the same place at which it was opened in Experiment 63, after which there was no motion in the lower extremities. The rabbit seemed lively after the operation, and continued to eat frequently till within six hours of its death. It died in twenty-seven hours and

a half after the division of the spinal marrow. It had not vomited, and had had little or no difficulty of breathing.

On examination after death, the stomach was not found more distended than natural. The food it contained was nearly as well digested as in the stomach of a healthy rabbit. The contents of the duodenum had completely undergone the proper change. The bladder and rectum were distended, but not so much as after the destruction of the lower part of the spinal marrow.

The lungs collapsed on opening the thorax, but contained a little frothy mucus.

On examining the spine, it was found to have been completely divided.

On the comparison of this experiment with Experiment 64, it appears that here the lower part of the spinal marrow still performed its office, and supplied its portion of nervous power to the stomach and lungs, although the communication between it and the brain was cut off.

The reader must have perceived through the whole of these experiments, that any considerable diminution of the nervous influence almost wholly deprives the stomach of its power, and even the slightest diminution of it is felt. I have no doubt that the very slight derangements observed in the stomach and lungs in the last experiment, may be ascribed to the destruction of function, that must have taken place in the part of the spinal marrow at which it was divided. The wound must of course have destroyed the function of a small part on each side.

Thus it appears that although we cannot by agents, applied to different parts of the brain and spinal marrow, ascertain, as was done with respect to the heart, how far the stomach and lungs are under the immediate influence of all its parts; yet, in consequence of the secreting power being under the influence of the nervous system, we may, by withdrawing that of different parts of the brain and spinal marrow, prove that the stomach and lungs, like the heart, are capable of being influenced through every part of those organs.

As the functions of the stomach and lungs depend on the secreting power, the same inferences must apply to it wherever it exists; and, consequently, as will appear more fully from what is said in a subsequent chapter, to the function of the vessels employed in the secreting process in all parts of the body.

CHAPTER XIII.

On the Cause of Animal Temperature.

The temperature of the animals in the preceding experiments, in which portions of the spinal marrow were destroyed, is now to be considered. It appears from them, that while the destruction of part of the spinal marrow impedes the office of secreting surfaces, it also lessens the temperature of the warm-blooded animal.

A little reflection will at once point out why the temperature should be diminished by destroying portions of the spinal marrow, although no such effect is produced, although the function of the stomach is still more effectually destroyed by removing portions of both of the eighth pair of nerves. By this means we take nothing from the active portions of the nervous system, which we have seen are confined to the brain and spinal marrow. Thus the power by which the temperature is maintained remains unimpaired; whereas, exactly as we lessen the extent of the spinal marrow we impair that power.

Sir Benjamin Brodie, in the Croonian Lecture for 1810, gave an account of experiments which led to the inference, that the maintenance of animal temperature is under the influence of the nervous system; and in the Philosophical Transactions of 1812, he relates additional experiments, tending to strengthen this inference.

In the second section of the last chapter, I have had occa-

sion to relate experiments made for other purposes, which tend in a striking manner to confirm the opinion of Sir Benjamin Brodie. He found that poisons, impairing the vigour of the nervous system, impair the temperature. In the foregoing experiments, lessening the extent of this system, by destroying part of the spinal marrow, had the same effect.

Towards the conclusion of the latter of the above papers, Sir Benjamin observes, "The facts here, as well as those formerly adduced, go far towards proving, that the temperature of warm-blooded animals is considerably under the influence of the nervous system; but what is the nature of the connexion between them? Whether is the brain directly or indirectly necessary to the production of heat? These are questions to which no answer can be given, except such as are purely hypothetical. At present we must be content with the knowledge of the insulated fact; future observations may, perhaps, enable us to refer it to some more general principle."

The various phenomena of animal temperature, and the experiments on this subject, related in the last chapter, compared with the effects on secreting surfaces, observed in these experiments, seem to me to prove, that animal temperature is maintained by the same means, namely, the action of the nervous power on the blood, by which the formation of the secreted fluids is effected.

I will relate some experiments on animal temperature, both when treating of the order in which the functions cease in dying, and in the observations which I shall have occasion to make on the nature of the vital powers, tending to confirm this view of the subject.

CHAPTER XIV.

Some additional Observations relating to the Function of Digestion.

Several physiologists[1] have related experiments which appeared to them to prove, that when after a part of the eighth pair of nerves is removed, and thus the due secretion of gastric juice prevented, it may be restored by mechanically irritating the cut ends of the lower portions of the divided nerves. If such be the fact, it must materially influence our views, both with respect to the function of digestion and the other secreting processes of the animal body.

In judging of the result of such experiments, several things must be taken into the account, which appear to have escaped the attention of those gentlemen.

At the time the animal is fed, in preparation for the experiment, there may be some food in the stomach, from previous meals more or less digested, and there is always some gastric juice ready to act on any new food which may be presented to it. It is evident, therefore, that although the secretion of the gastric juice cease at the moment of the excision of part of the eighth pair of nerves, some digested food must be found in the stomach for some hours after the operation; for, as I have ascertained by numerous trials, many hours are required in such experiments for the stomach to propel into the intestine the remains of food previously digested, or that digested by the gastric juice previously formed.

[1] See a paper entitled, Mémoire sur la Mode d'Action des Nerfs Pneumogastriques dans la Production des Phenomènes de la Digestion. Par M. Breschet et Milne Edwards. (Lu à la Société Philomatique, le 19 Fevrier 1825.)—Extrait des Archives Generales de Medecine.

When, therefore, the contents of the stomach are examined in five or six hours, and generally even in ten or twelve, after the operation, more or less digested food is found lying next the surface of the stomach. But when the animal survives the operation eighteen, twenty, or more hours, undigested food alone is found in it. The cause of so long a time being required wholly to expel the food, which has undergone any degree of the digestive process, appears to be, that as digested food alone excites that action of the stomach which propels it into the intestine, and the more perfectly it is digested, it excites this action the more readily, those parts of the digested food which have but imperfectly undergone the digestive process are expelled very slowly, so that it is very long before food wholly undigested alone is left.

That the longer the animal lives after the excision of part of the eighth pair of nerves, the less digested food is left in the stomach, is a fact now admitted by all who assisted at the experiments. Among the great number who have witnessed and been satisfied with their result, are Sir Humphrey Davy, Sir Benjamin Brodie, and Mr. Thomas Andrew Knight, gentlemen whose experimental accuracy, in the opinion of the public, has never been surpassed.

Of this fact, the gentlemen to whose paper I have referred are not aware. They maintain, indeed, that the only effect on the digestive process produced by the excision of part of the eighth pair of nerves, is, that it becomes more tedious, being as perfect as when the nerves are entire, if a sufficient length of time be afforded. In speaking of the animals in which part of the eighth pair of nerves had been cut out, and comparing them with the healthy animal, they say: "Enfin, si on laisse écouler un espace de temps plus grand encore entre l'operation et la mort des animaux, on pourra trouver que la digestion est complétement achevée dans l'un comme dans l'autre cas"—a statement exactly in opposition to the fact, because every hour the animal

lives after the operation, renders the proportion of digested food remaining in the stomach less.

It will easily be perceived to what errors, respecting the effect on digestion, of depriving the stomach of the office of the eighth pair of nerves, this misconception, which probably arose from the animals not having survived long enough to allow of the digested food being expelled from the stomach, must lead. Its effect was increased by the different animals in each experiment having been confined to the same quantity of food. The most hungry would of course digest it fastest and most perfectly, because in them there would be the greatest quantity of gastric juice collected in the stomach before the excision of part of the nerves. To judge fairly of the result of the experiment, the different animals must be allowed equally to satisfy their appetite, to eat till, from their manner of eating, it is found that the appetite has equally abated in all. It will be found, I think, that several other experimentalists have allowed themselves to be deceived by the circumstances here enumerated. We thus readily account for the discrepancies of writers. MM. Leuret and Lassaignec state that in horses, eight hours after five or six inches of the eighth pair of nerves were cut out, the digestion was going on as usual;[1] while Mr, Field, whose experimental accuracy is generally acknowledged, and in whose experiments the same animals (horses) survived many times eight hours, found nothing in the stomach but undigested food covered with what appeared to be a layer of mucus, which I have often seen under the same circumstances in other animals; and which arises from that effort of nature to defend the surface of internal cavities against uncongenial contents, for which among other reasons they are lined with the mucus membrane.

Such are the circumstances which I conceive misled those gentlemen who maintain that they can produce a sensible effect

[1] Outlines of Human Physiology, by Herbert Mayo, F.R.S., second edition, pages 184 and 185.

on the contents of the stomach by mechanical irritation of its nerves.

They also err in supposing that the muscular fibres of the stomach can be excited by irritating the eighth pair of nerves in the way that a muscle of voluntary motion may be excited through its nerves. The digested food is the natural stimulus of the muscular fibres of the stomach in its usual function, as the nervous power is of the muscles of voluntary motion in theirs; and we cannot through the nerves excite the former as we do the latter class of muscles. The muscular action of the stomach resembles that of other hollow muscles, in being excited by its contents, and, if at all, only occasionally and under peculiar circumstances, as in the case of the heart, by the nervous influence.

Hence, as I have already had occasion to observe, depriving the stomach of the nervous influence has no effect whatever in preventing its propelling the digested part of the food into the first intestine. MM. Leuret and Lassaignec observe, that the only obvious and necessary effect of the operation is to paralyse the sphincter muscle of the cardiac orifice of the stomach, an effect of which, in all the experiments I made, I never saw any sign, and which, as far as I know, no other experimentalist has mentioned, for food being found in the œsophagus is no proof whatever of such an effect; and its extreme improbability, while we have undeniable evidence that the other muscular powers of the stomach are entire, is apparent.

The mechanical irritation employed by MM. Breschet and Milne Edwards, in endeavouring to excite the digestive process after a portion of the eighth pair of nerves had been removed, was that of a thread attached to the cut extremities of the lower portions of the eighth pair of nerves, and fastened to the neighbouring muscles, by which the motions of respiration kept the part in a constant state of irritation.

A similar experiment is related above, in which the cut extremities of the lower portions of the nerves were fastened to a

thread tied round the neck of the animal, by which it was in like manner kept in a state of constant mechanical irritation; yet in the stomachs of the animals after they had lived more than twenty hours,—for the experiment was made more than once,—nothing but undigested food was found. This experiment, with some others connected with it, was made publicly in the rooms of the Royal Institution; and all who felt an interest in the subject were admitted to see the results, nor was there one who expressed a doubt respecting them.

As, however, in these experiments the position of the nerves was more disturbed, and the thread was not applied as in the experiments performed at Paris, Mr. Cutler, at my request, was so good as to make the following experiment.

Exp. 69. Three rabbits, after a fast of the same duration, were fed in the same way. In two of them a portion of each of the eighth pair of nerves was removed. The third rabbit was left undisturbed. In one of those in which the portions of nerve were removed, the cut end of the lower part of the nerves was by means of a bit of thread fastened to the neighbouring muscles, as in the experiments referred to. This rabbit died in ten hours, at which time the others were killed in the usual way.

M. Cutler then took out the stomach of all of them, slit them open, and laid them on the same plate; and Sir Benjamin Brodie was requested to examine and give his opinion respecting their contents, without having been told which was which. He at once pointed out the healthy stomach, the whole contents of which had undergone the action of the gastric juice. After carefully examining, and with an instrument moving about the contents of the other stomachs, he declared he could discover no difference in them. Both stomachs were chiefly filled with undigested food, the animals not having lived long enough after the operation for the expulsion of some imperfectly digested food that still remained in both.

The foregoing experiment convinced those who witnessed the

result, that the irritation caused by the attachment of the cut end of the nerves to the muscles, had, as might well be expected, no effect whatever in promoting the digestion of the food.

Even were it possible, as in the case of the nerve of a muscle of voluntary motion, mechanically to excite the eighth pair to perform its office after its communication with the brain is wholly intercepted, it is surely impossible that this could go on for many hours, which are necessary for the digestion of food. A nerve of voluntary motion, if kept in a state of excitement after its separation from the brain and spinal marrow, loses its power in a very short time, at most a few minutes.

The result of the foregoing experiment may be known before the death of the animals. It is clear that if the power of the nerve be restored, its restoration must be as evident in the function of the lungs as in that of the stomach, the functions of both equally depending on these nerves. In the foregoing experiments, both the animals, in whom part of the nerves had been removed, were affected with extreme dyspnœa, the mechanical irritation of the nerves having no more effect in relieving this symptom, than in promoting the due action of the stomach.[1]

CHAPTER XV.

A Review of the Functions of the Nervous System, and of the Relation which they bear to the other Functions of the Living Animal.

The importance of the nervous system in the animal economy is such, whether we look to a correct view of its healthy functions, or to a clear understanding of their various deviations in disease,

[1] The foregoing facts and observations were published in the Philosophical Transactions for 1825, and, as far as I know, no reply has in the lapse of fourteen years been made.

that it will place in a clearer light both the remaining part of the present, and the whole of the last, the practical, part of this volume, to present at one view its various functions, and the relation they bear to the other functions of the more perfect animal.

The nerves may be divided into two classes, those which proceed directly from the brain and spinal marrow to the parts to which they convey the influence of these organs; and those which enter such ganglions as receive nerves proceeding from different parts of the brain and spinal marrow, whether these nerves have or have not protuberances belonging to themselves, which have also been termed ganglions, but which receive only the different fibres that belong to the particular nerve to which they are attached, and from the circumstances in which they are placed, must have a different, or at least a more confined relation to other parts of the nervous system. To the former, therefore, I shall, for the sake of distinction, and to avoid circumlocution, confine the word ganglion.

I beg leave to lay before the reader the following extract from lectures delivered by Sir Benjamin Brodie before the College of Surgeons, and which have not yet been published, in which this eminent anatomist and physiologist has given the sum of our knowledge respecting the structure of the ganglions.

"Those bodies which are found in certain nerves which appear to be formed by an enlargement of the nervous substance, and which are denominated ganglia, are of a complicated structure. Into ganglia the nervous fibres may be traced, and from these ganglia the nervous fibres again emerge. Scarpa has paid much attention to the fabric of the ganglia, and he gives the following history of it. He says that the fasciculi of nervous filaments which enter a ganglion are separated and divided from each other, and that they are combined anew. A nervous fasciculus entering a ganglion divides into smaller fasciculi. These divide again, and cross and intersect each other at various angles. Then the divided fasciculi become again united,

and as at first they divided into smaller and smaller fibres, so, when they begin to unite, they form gradually larger and larger bundles. At last the nerve which entered a ganglion emerges from it with its fibres collected into one or more fasciculi. Sometimes several nerves enter a ganglion, in which case they are all blended together, forming a complicated net-work, in which it is impossible to determine what belongs to one nerve and what belongs to another nerve. Every fasciculus or filament which enters a ganglion passes through it. There is no appearance of any one terminating in it.

" If we unravel the texture of a ganglion, we find that each nervous fibre retains its own peculiar neurilema; but besides this, the spaces left between the intersection of the fibres are filled up with a peculiar soft substance of a greyish or yellowish colour. With the nature of this substance we are unacquainted. Some have considered it as corresponding to the cineritious substance of the brain and spinal marrow; but Scarpa is disposed to regard it as a soft cellular substance, filled with a greyish and mucilaginous matter in emaciated subjects, and with a yellowish oily matter in those that are fat."

Such then is the structure of the ganglions, as far as it is known; and as, for the reason just mentioned, I shall confine the term to those ganglions which receive nerves proceeding from different parts of the nervous system; the term ganglionic nerve I shall confine to those nerves which either enter or proceed from such ganglions, without adverting to their having or not having protuberances resembling ganglions belonging to themselves. It is necessary to keep this explanation in view, because neither the term ganglion nor ganglionic nerve has been employed with much precision.

Physiology has been greatly indebted to the experiments of Sir Charles Bell, M. Magendie, and Mr. Mayo, for a knowledge of the different functions of the two sets of nerves which form the origins of the spinal nerves, and of certain branches of the fifth and seventh cerebral nerves. It appears from their

experiments, that one of the foregoing sets of nerves, with the facial branches of the fifth cerebral nerve, are nerves of sensation; the other set, with the portio dura of the seventh cerebral nerve, of voluntary motion; facts which explain many of the phenomena of disease, which had suggested the probability of these functions being exercised by different nerves bound up in the same envelope. Dr. Parry, in his treatise on the pulse, for example, relates a case where feeling alone was lost in one arm, and voluntary power alone in the other.[1] But these are not the only, nor indeed, as far as life is concerned, the most important functions of the spinal nerves. All of them contribute to the formation of the ganglionic system, on which the life of the animal, as appears from many facts stated in the preceding parts of this volume, immediately depends, this system being as essentially a vital organ as the heart or lungs.

It is evident from what has been said, that the ganglions and nervous plexuses, which accompany them, resemble each other in their nature; and as the nerves which terminate in them come from all, even the most distant parts of the nervous system, some from the brain, and some from the lower extremity, and all intermediate parts of the spinal marrow, we cannot help supposing that there is some design in thus uniting nerves from so many sources. One of the most striking differences between the ganglionic nerves, and those proceeding directly from the brain and spinal marrow, is, that even independently of the ganglions and plexuses, the former everywhere freely anastomose, if I may borrow a term from the sanguiferous system; while the latter proceed in a more direct course, being less connected with each other in their progress, to the parts on which they bestow sensation and voluntary power; still further demonstrating the care with which Nature blends the ganglionic nerves. There is even reason to

[1] Independently of such phenomena, it seemed difficult to believe that sensation and voluntary power, often simultaneous functions, in the same parts, belong to the same nerve, as they evidently depend on impressions conveyed in opposite directions.

believe that the protuberances resembling ganglions, belonging to individual nerves, serve the purpose of intimately combining the influence of the different fibres of the nerves they belong to, and that all the nerves having such protuberances contribute to the ganglionic system.

What purpose is served by the perpetual intertwining of these nerves? It is impossible for a moment to conceive that it is without an object.

This question is most likely to be answered by inquiring into the nature and functions of the parts supplied by them; those parts are the vital organs—namely, the thoracic and abdominal viscera, and the vessels even, as we find by experiment, where the parts are too minute to be made the subject of dissection, to their minutest ramifications.

If the nerves proceeding from ganglions convey the influence of all the nerves which terminate in them, it would seem that, although to other parts the influence of only certain parts of the brain or spinal marrow is conveyed, the vital organs receive that of every part of them. The question can only be determined by experiment. That it must be answered in the affirmative appears from numerous experiments, an account of which has been laid before the reader, and which are too simple to admit of our being deceived in their results.

From them it appears that although the muscles of voluntary motion obey an agent affecting no part of the brain and spinal marrow but that from which their nerves take their origin, the heart is influenced by agents applied to every part of these organs, from the very uppermost surface of the brain and cerebellum to the lowest portion of the spinal marrow. The same was found to be the case with the blood-vessels to their minutest ramifications. Even the extremities of the arteries and veins, where they unite to complete the circulation, it was found, by the aid of the microscope, could be influenced, (their action being either increased or impaired according to the nature of the means employed,) nay, even finally deprived of power, by agents whose

operation was confined either to the brain or spinal marrow. On the power of the capillary vessels we have seen the circulation in a great degree depends.[1]

In some animals even of warm blood, as appears from the foregoing experiments, the motion of the blood in the capillaries may be observed for two hours or even more after what is called death, provided neither great and sudden injury to the nervous system, nor great loss of blood, be occasioned by the mode of death; that is long after the heart has ceased to beat. The continued action of the capillaries appears, from what is there said, to be the cause of the large arteries being found empty some hours after death, for the motion of the blood is maintained in them long after the arteries can give it no assistance except by the elasticity of their coats.

It has also been shown by direct experiment, that the stomach and lungs are in like manner under the influence of every part of both the brain and spinal marrow, their functions finally ceasing when they are deprived of any considerable portion of the influence they receive from either of these organs.

The partial connexion with the nervous system of the organs supplied by the cerebral and spinal nerves, and the universal connexion with that system of those supplied by the ganglionic nerves, explain many of the phenomena both of health and disease. Why are affections of the stomach and other vital organs instantly felt through every part of the frame, while the effects of those of a muscle of voluntary motion, or even an organ of sense, although often a part of greater sensibility, is confined to the injured part? If the eye or ear, or the muscle of a limb, be so deranged by a sudden blow, for example, as instantly to destroy its power, sight, hearing, or the voluntary power of the part is lost, and there the evil ends unless inflammation ensues; but a blow on the stomach, which instantly destroys its power, at the same

[1] See the chapter *On the Sources and Nature of the Powers on which the Circulation of the Blood depends.*

moment destroys that of every other part. It is not difficult to answer the question, since the state of the stomach, from the cause just pointed out, may influence every part of the nervous system ; and it appears from experiments, which have been laid before the reader, that a powerful and sudden impression, made on any considerable part of this system, is capable of destroying the circulation by instantly depriving both the heart and blood-vessels of their power.

Here the question naturally arises—For what purpose are the vital organs thus connected with every part of the brain and spinal marrow?

This question is answered by experiments which have been laid before the reader. From them it was found that the power of secreting surfaces is deranged by abstracting from them any considerable part of the influence either of the brain or spinal marrow ; and as the function of secretion is effected by the action of the nerves on the blood, as appears from all the facts relating to the subject which have been laid before the reader, it is evident that the presence of nervous power in a secreting organ would be useless, were not the blood on which it operates also supplied, and disordered if it were not supplied in due proportion ; and consequently its supply varies as the supply of nervous power varies.

We thus see not only why secreting surfaces are placed under the influence of every part of the nervous system, but also why it is necessary that the sanguiferous system should be under the control of the same laws which regulate the supply of nervous power.

If the nervous influence of the thoracic and abdominal viscera be thus supplied from a common source, why, in affections of the spinal marrow, it may be said, is the breathing most affected when the disease is in the dorsal portion of this organ, and the action of the bladder and rectum most affected when its chief seat is in the lumbar portion? This arises from the muscles of respiration deriving their nerves from the dorsal portion, and the

abdominal muscles from the lumbar portion of the spinal marrow. The latter muscles excite, and generally increase, the action of the bladder and rectum, by pressing them against their contents, and also when required by the diminution of their usual excitability, by this pressure contribute mechanically to expel their contents. Thus, in the above cases, in addition to the failure of nervous power in the viscera, there is a failure of excitement in the muscles of voluntary motion, which conspire with these viscera in certain parts of their functions.

We can trace the communications of nerves issuing from the great chain of ganglions, placed, it would seem, to facilitate these communications in the centre of the animal system, with all the nerves of the body. And many circumstances, regarded by anatomists as anomalous, namely, nerves becoming larger after they appear to send off branches, apparently taking a retrograde course, &c., are readily explained, if we admit that nerves, arising from ganglions, join and again separate from those proceeding in an opposite direction from the brain and spinal marrow. It is worthy of remark that none of these anomalous appearances are observed in the extremities, where the ganglionic must take the same course with the other nerves. Bichat, although his opinions respecting the use of the ganglions are very different from those which I have, from the results of the preceding experiments, been led to form, and indeed wholly inconsistent with their results, was induced from his observation of the situation and distribution of the ganglions and their nerves, to regard them as the centres of nervous systems.

The increase of the secreting power in any part, it is evident, would be vain, were there not at the same time a corresponding increase in the supply of the fluids on which it operates. A similar observation applies to the excretory muscles as far as they are muscles of involuntary motion. The same increase of nervous influence which occasions an increased flow of secreted fluids, excites these muscles to carry off the increased quantity.

Nature does not seem on such occasions to trust this to the increased stimulus occasioned by the increased flow of the secreted fluid, which we have reason to believe, from the *modus operandi* of certain causes of inflammation, would often occasion morbid distension. Now, the vascular system and the muscles of excretion, if in them we include the alimentary canal, comprehend all the muscles which are supplied with nerves from the ganglions.

Thus we see the necessity of every part relating to the function of secretion which the ganglions appear to perform. A combination of the whole nervous influence is necessary for the due formation of the secreted fluids; and that there may be, under all circumstances, both a due supply of the fluids to be acted upon, and a due removal of those prepared, whether for the functions of life or for the purpose of being thrown out of the system, it is necessary, as appears from what has just been said, that the powers which convey these fluids should be subjected to the influence by which secretion is performed.

The constant presence of fluids in secreting surfaces solicits a continual supply of nervous influence to them, so that they go on during our sleeping as well as waking hours. The more copious the supply of fluids to secreting surfaces, we find the secreting power the greater, and *vice versâ*. The function of secretion, it is evident, requires a more regular supply of fluids than could have been obtained had the usual action of the vessels depended on the nervous system, which is subject to continual variation; but had not this system been capable of influencing the vessels, no change could have influenced the flow of secreted fluids. Thus it is necessary that the power of the sanguiferous should be independent of the nervous system, yet capable of being influenced by it; as it is ascertained to be by the experiments related in the first and second chapters of the present part of this Treatise.

That the reader may see how far the observations of the anatomist correspond with the preceding experiments, he need only consult any of our systems of anatomy; by which he will find that the ganglions may receive the influence of every part of

the brain and spinal marrow, and communicate that influence to every part of the body.

It appears from all that has been said, that while, through the cerebral and spinal nerves, the animal is connected with the external world, the ganglionic system is strictly a vital organ. It has no object but that of maintaining the functions necessary to life.

It seems to be superfluous, after the experiments which have been related, to say anything in refutation of the opinion of Bichat, that the ganglions are centres of nervous influence, independent of the brain and spinal marrow. "Les nerfs des ganglions ne peuvent transmettre l'action cérébrale; car nous avons vu que le système nerveux, partant de ces corps, doit être considéré comme parfaitement independant du système nerveux cérébral; que le grand sympathique ne tire point son origine du cerveau, de la moelle épinière ou des nerfs de la vie animale; que cette origine est exclusivement dans les ganglions; que ce nerf n'existe même point, à proprement parler, qu'il n'est qu'un ensemble d'autant de petits systèmes nerveux; qu'il y a de ganglions, lesquels sont des centres particuliers de la vie organique, analogues au grand et unique centre nerveux de la vie animale, qui est le cerveau."—*Recherches Physiologiques sur la Vie et la Mort, par Xav. Bichat*, page 355 et seq.

It appears then that by means of the system of ganglionic nerves, the influence of every part of the brain and spinal marrow is bestowed on secreting surfaces, and on those organs by which the supply of their fluids is regulated, and that the influence of every part of those organs is necessary to their functions. But it is not the secreting power alone that is thus placed under the influence of every part of the brain and spinal marrow; for it is a necessary inference from experiments detailed above, and other statements which have been laid before the reader, that the whole of those processes on which the healthy structure of our organs depends are under the same influence.

The influence therefore of the whole brain and spinal marrow

is thus united by nerves arising from every part of these organs entering ganglions and plexuses, from which are sent to every part of the body, nerves proved by direct experiment to convey the influence of every part of them; and this combined influence of the brain and spinal marrow is employed in maintaining all the assimilating processes and forming the various secreted fluids, and I have in more than one treatise in a general way pointed out how extensively the phenomena and cure of diseases are influenced by this cause, and shall for the first time in the following part of this volume enter at length into the various inferences arising from it—the most important of all the practical inferences from the present investigation.

Such then is the relation which subsists between the nervous system and those organs on which the functions of life immediately depend; but there is another relation of that system which must be investigated before the nature of its functions can be clearly understood.

The nervous system, in the usual acceptation of the term, is very ill defined, and functions of the most dissimilar nature are classed together under the general denomination of nervous. Those of sensation and volition, for example, are classed with the excitement of a muscle and the formation of a secreted fluid. It is impossible to believe that results so different in their nature should arise from the same or similar causes. On the most cursory view of the subject, we cannot help supposing that the nervous system, according to the common acceptation of the term, includes more than one principle of action. We have every reason to believe, that the sensorial is a power wholly distinct from that strictly called nervous; and all doubt seems to be removed by the results of the experiments which have been laid before the reader, from which it appears that although the organs of both belong to the nervous system, it is evident they are not the same organs, because the sensorial power, particularly in man, resides chiefly in the brain, while the nervous power, properly so called, resides equally in the brain and spinal marrow; the latter

of which organs is capable of its functions independently of the former, as appears from many of the experiments above detailed.

It occurred to me, on reviewing the whole of these circumstances, that as we can destroy the nervous, without at all impairing the muscular power, it might be possible to remove the sensorial power, without immediately destroying that more strictly called nervous, and thus afford an unequivocal proof of the distinct origins of these powers.

CHAPTER XVI.

On the Sensorial Functions.

A VIEW has now been presented to the reader of the general laws of the muscular and nervous functions, and the means by which these laws have been ascertained.

A set of functions still remains to be considered, which, as far as I can judge, will be found equally distinct from both of these, although they have never been correctly distinguished from the nervous functions, and the maintenance of which seems to be the final cause of both the others. The nervous and muscular functions maintain the life and health of the animal, and are the immediate means of intercourse between it and the external world; by the sensorial functions it is rendered capable of enjoyment, and the immediate means of intercourse with the external world are regulated. Were this, however, their only object, they would not fall within the scope of the present treatise, which is confined to the vital powers.

According to generally received opinions, therefore, except as far as the sensorial powers are necessary for obtaining a supply of food, they form no part of the subject of this Inquiry. I have, however, been led by it to a conclusion, in which I confidently

expect the concurrence of the reader, when the whole of the facts shall have been laid before him, that two of the sensorial functions, sensation and volition, are immediately necessary to life in the perfect animal.

Nothing can be more indistinct than the opinion of physiologists respecting the powers by which life is maintained. Constantly engaged in the vain attempt of ascertaining what life is, they have been less anxious to trace the causes on which its preservation immediately depends, an object open to experiment, and, even if the other were attainable, one of far greater importance.

It was at once evident that the muscular power is essential to the continuance of life; but it will be admitted that any ideas which have been entertained respecting the share contributed by the nervous system towards this end, have been extremely vague;[1] and the sensorial functions, except in the indirect way just mentioned, have been regarded as out of the question.

It appears to me that what I am about to say of these functions cannot be better introduced and connected with the preceding part of the Inquiry, than by the following observations on the division of the muscles into those of voluntary and involuntary motion.

A great variety of opinions have prevailed respecting the cause of some muscles being subjected to the will, while others are independent of it. One of the most plausible, and which professes to be the result of experiment, is that of Dr. Johnstone,[2] "that the ganglions are the instruments by which the motions of the heart and intestines are from the earliest to the latest periods of animal life rendered uniformly involuntary."

Dr. Johnstone's experiments, an account of which is given in the 25th and following pages of the work just quoted, and other

[1] In the latest and best accounts of the present state of our profession, we have seen it taught that the nervous system has no share in the maintenance of life.

[2] Med. Essays and Observations, 1795.

experiments of a similar nature to which he refers, of Haller, Whytt, and others, were made with a view to prove that it is impossible to affect the action of the heart by stimulants applied to the brain and spinal marrow.[1] These physiologists appear to have been deceived by the following circumstances. They did not employ the precaution of preventing the action of the muscles of voluntary motion, which renders it impossible to judge of the effect of the stimulant on the heart; and they were not aware that the heart will not obey a stimulant applied to the brain and spinal marrow, however powerful, unless it be applied to a portion of considerable extent. They were probably deceived also by expecting to see in the heart the irregular motions excited by artificial stimulants in the muscles of voluntary motion. Any person who attends to these precautions, will find that the heart is not only as easily stimulated through the brain and spinal marrow as the muscles of voluntary motion are; but that it may be stimulated through them in the newly-dead animal for a considerable time after these muscles can no longer be influenced in this way; proving that the ganglions oppose no obstacle to the influence of the brain and spinal marrow being extended to the muscles of involuntary motion.

These muscles being exposed to the constant or constantly renewed effects of stimulants, over which the will has no power, in a great measure accounts for their action being involuntary. What power of volition could prevent the blood from exciting the contractions of the heart? But the writers on this subject, as far as I can judge, wholly overlook the point of most consequence in determining the question, namely, that the action of the muscles of involuntary motion can effect no end desired. We will to move a limb, not to excite a muscle. We wish to handle, for example; and, on trial, find that we can move the fingers; but what act of volition could we perform through the medium of the heart or blood-vessels? If we had had no wish to handle, the

[1] This opinion was maintained by several authors before Dr. Johnstone and the other authors here mentioned; but, as far as I know, they are the only ones who made experiments with the view to support it.

muscles of the fingers, of course, would never have become subject to the will. Few have any command over those of the external ear, the position of the human ear but little influencing the sense of hearing. The animals whose ears are so shaped that their position materially assists the sense, move the external ear as readily as any other part of the body. It deserves to be remarked, that the will influences the lowest part of the intestines and bladder, the only internal organs which can assist in accomplishing an end desired.

Such is the first instance in which we have found the sensorial power, through the nervous and muscular powers, directly influencing the organs of life. We shall find it through the same channels influencing them in a function of far greater importance.

CHAPTER XVII.

On the Relations which the Vital Functions bear to each other, and the manner in which they partake of the Sensorial Functions.

That the reader may follow me in my endeavours to ascertain the degree and manner in which the sensorial functions are necessary to life in the more perfect animals, it is requisite that he should bear in mind the result of what has been laid before him respecting the muscular and nervous functions.

It appears from the preceding experiments and observations, that the muscular power resides in the muscle itself; that the influence of the nervous system, whatever that may be, stands in no other relation to it but that of a stimulant, and that this power is the same in all the muscles, the means of exciting it alone varying in those of voluntary and involuntary motion.

With respect to the nervous system, he has seen that the nerves afford the sole stimulant of the former, and an occasional stimulant of the latter set of muscles, and are the means of conveying impressions to and from its own more central parts, the brain and spinal marrow; there being no evidence that impressions are ever communicated from one nerve to another, independently of the intervention of one of these organs.[1]

The nervous system also, as appears from the statements which have been laid before the reader, maintains by its action on the blood the assimilating and secreting processes, on which the structure of our various organs depends; and it appears from the facts which have been adduced respecting animal temperature, is the means which supports the temperature necessary to life in the more perfect animals.

The subject of the present chapter may be divided into two parts. In the first I shall attempt, by the aid of experiment, to draw a correct line of distinction between the sensorial and nervous functions; and in the second, by the same means, to trace the manner in which all the functions are so connected as to render the sensorial power essential to the continuance of life in man and the animals which resemble him.

The seat of the sensorial and nervous powers has not been so well defined as that of the muscular power. M. le Gallois appears to regard the brain as the seat of the one, and the spinal marrow, of the other; but many observations are in opposition to this opinion; nerves proceeding wholly from the brain exhibit the phenomena of nervous power properly so called; and that the spinal marrow possesses sensorial power, may be made apparent by very simple experiments.

Exp. 70. Even in the warm-blooded animal, the rabbit for example, after all feeling, as far as the brain is concerned, is destroyed, when one of the limbs is wounded, the others are moved, demonstrating that it still possesses the means of receiving the impression from one set of nerves and communicating it to an-

[1] See the Observations on the Sympathy of Nerves.

other. In the cold-blooded animal, the same thing is observed in a greater degree. For a considerable time after the head is cut off, the frog will sometimes even sit in its usual position, and appear sensible to an injury inflicted on any part of it.[1] It is evident from many observations, however, that the sensorial power chiefly resides in the brain, and that the power possessed by the spinal marrow is chiefly nervous.

If these powers, it may be said, are thus blended in their organs, what proof have we of their being distinct powers? This proof, it appears to me, will be found in carefully observing the process of dying, of which what we call death is only the first stage.

However blended the organs of the sensorial and nervous powers may appear to be, we are assured that they are distinct organs, by the fact that while those of the nervous power evidently reside equally in the brain and spinal marrow, those of the sensorial power appear to be almost wholly in man, and chiefly, in all the more perfect animals, confined to the former. It may be possible, therefore, to withdraw the power on which the one set of functions depends, without immediately destroying the other, as we find we can withdraw the influence of the nervous from the muscular system without destroying the power of the latter.

At the instant of death, it is evident the sensorial functions cease, no impression is perceived or followed by any act of volition. It is, however, equally evident to the physiologist, that

[1] These experiments, made five-and-twenty years ago, exhibit instances of what has lately occasioned so much discussion under the term reflex action of the nerves. They wholly depend on the spinal marrow in certain animals more or less partaking of the sensorial power. In the nervous system properly so called, there neither is, nor can there be, any such property. Such a supposition is one of the many inaccuracies which have arisen from a correct line of distinction never having been drawn between the sensorial and nervous functions. This property of the spinal marrow is always found exactly in proportion as it partakes of the sensorial power.

the muscular power still remains. If under these circumstances the heart or muscles of voluntary motion be stimulated, they still possess the power of contraction, which is only lost by degrees, and not till after the sensorial power has for a considerable time been withdrawn.

It is also evident to the physiologist, that some part of the nervous power still exists, for if the nerves themselves, or those parts of the brain or spinal marrow from which they originate, be irritated, the corresponding muscles are thrown into action. The nerves, therefore, are still capable both of conveying impressions and exciting the muscles. Are they capable of their other functions? Can they effect the formation of the secreted fluids, maintain the various processes of assimilation, and raise the temperature of the blood after the sensorial power is withdrawn?

I have already had occasion to refer to Mr. Hunter's observations respecting the digestion of the stomach after death. It is perhaps superfluous to observe, that this is not to be regarded as a vital action. It is a mere chemical process. But Mr. Hunter, as appears from the following observations, suspected that a truly vital action continues in the stomach for some time after what is called death. "This is exactly the case with the experiments of Spallanzani, which, although they prove that meat was digested in the stomach after the animal was killed, which no one doubted," that is, no one doubted that the gastric juice already in the stomach would continue to perform its office there, "yet are not at all calculated to show that the stomach itself may be digested. In fact, the manner in which they were managed rather tended to prevent that effect from taking place; the gastric juice, having substances introduced on which it could act, was less likely to affect the coats of the stomach.[1] That the digestion was not carried on merely by the effects of the gastric

[1] This observation, as appears from what is said above, is incorrect. It is soon after a full meal that the stomach is most apt to be digested after death, the cause of which I had occasion to explain.

juice secreted before death is evident from his own account, some of the food which had been introduced and digested being found in the duodenum; a thing which could not have happened if a cessation of the actions of life in the involuntary parts had taken place when visible life terminated. There had been an action, *and most probably a secretion in the stomach.*"[1]

It appeared to me that the question might be reduced to the test of experiment, by dividing, immediately after death, the eighth pair of nerves in the neck, which impairs, and in the newly-dead animal would probably destroy, the formation of gastric juice.[2] We are not, it is evident, to expect that any great secretion of gastric juice can take place after death, or consequently that any great difference can be observed between the food in the stomach of an animal in which the eighth pair of nerves have been divided immediately after death, and one in which they are left entire; and many circumstances which we cannot estimate, particularly there being more gastric juice in the stomach of the one animal than the other at the time they are killed, or one having eaten more than the other, must influence the result. It will not answer the purpose, it is evident, to confine the animals to the same quantity of food, because the stomach of the animal which is most hungry will digest it most quickly. The quantity of old food in the stomach also influences the result. The question, therefore, can only be determined by making the experiment on a large scale, to which, as it is made on the dead animal, there can be no objection.

Exp. 71. This experiment was made on twenty-six rabbits; eight full grown, eight half grown, six two months old, and four one month old. They were made to fast for sixteen hours, at the end of this time allowed to eat as much cabbage as they chose, and then killed in the usual way. Immediately after

[1] Observations on the Animal Economy.

[2] The author was not aware, when this experiment was made, that the mere division of these nerves will not wholly prevent its formation in the living animal, as will soon be more fully explained.

death, the eighth pair of nerves was divided in one half of those of each description, and they were all allowed to lie undisturbed for about twenty-two hours. The stomachs were then laid open, and those of the rabbits of the same age, who had eaten most nearly the same quantity, were compared together. The result was, that in twelve pairs the food was most digested in those animals whose nerves were left entire. In one pair it was most digested in the animal whose nerves had been divided. In several of those whose nerves had been divided, the cabbage appeared quite fresh and green. This did not happen in any whose nerves were left entire. In these the colour was always changed more or less to a brown. The difference in the state of the cabbage was sometimes more sensible to the touch than to the eye, that least digested feeling hardest. This experiment, at the same time that it proves the continuance of secretion after the sensorial power has ceased to exist, shows more than any experiment made on the living animal could do, how quickly the formation of gastric juice is impaired by the division of the eighth pair of nerves in the neck.

It is remarkable that the division of these nerves immediately after death almost always produced the same appearance of dark-coloured patches upon the surface of the lungs, but generally in a less degree, observed from it when the operation had been performed during the life of the animal; an effect, demonstrating that not merely the secreting power, but along with it, the whole of the assimilating powers remain after what we call death. These patches now and then appear in the lungs of an animal whose nerves are entire, after it has lain dead for some time; but much less frequently, and to a much less degree, than when the nerves have been divided immediately after death.

The appearance of dark-red patches on the surface of the lungs, the reader has seen, is always observed to a great degree when the eighth pair of nerves has been divided during the life of the animal, and it has survived the operation many hours. It may be regarded, and is mentioned by various physiologists, as

the characteristic effect of the operation on this organ. The obstruction of the lungs, which is also an uniform consequence of it, appears under many other circumstances, but I know of no other in which there is an appearance like this patching, except as I have just mentioned, that a certain degree of it, or rather something like it, now and then appears in the lungs of the entire animal after it has lain dead for many hours. In the experiments in which the eighth pair of nerves were divided in the living animal, it was always proportioned to the degree in which the secreting power of the lungs was deranged, that is, to the failure of nervous influence ; and it appears from direct experiments, which have been laid before the reader, that it always indicates a complete failure in the part of all the assimilating powers. Thus it appears, from the experiment just detailed, that the whole of the assimilating powers remain after the sensorial power no longer exists.

In considering the result of the foregoing experiment, the question arises, What occasions any supply of fluids to secreting surfaces, after the circulation has ceased, and thus enables the remaining nervous influence to produce any secreted fluid after death? The result of the following experiments appears to afford a ready answer to this question.

Exp. 72. A rabbit, about two months old, was killed in the usual way. The chest was then laid open, and a ligature thrown round the aorta. Part of the mesentery was now brought before a microscope, and the blood in its vessels seen, both by Mr. Sheppard and myself, moving with great velocity. By examining different parts of it, and choosing those which had not been previously disturbed, and consequently still retained some warmth, we found the circulation, in the smaller vessels, going on with rapidity for a quarter of an hour after the aorta had been secured, and an irregular motion of the blood in these vessels was evident for twenty minutes longer, the blood stopping and going on, and sometimes moving backwards and forwards in the same vessel. This could be distinctly seen long after the part had become

quite cold. This experiment was performed in the sunshine, in the open air, where there happened to be a good deal of wind, and the exposed part of the mesentery quickly became parched; which, as we found from other trials, destroyed the motion of the blood in the capillaries long before it naturally ceases.

Full-grown rabbits are bad subjects for this experiment, on account of an accumulation of fat which takes place in the mesentery and obscures the vessels. Rabbits about six weeks old, when they have been fed for some time on green meat, are generally thin, and consequently the best subjects for it.

Exp. 73. A dead rabbit, about a month old, whose intestines had been submitted to examination, after a ligature had been thrown round all the vessels attached to the heart and this organ removed, was thrown aside. An hour and a quarter after the heart had been removed, I brought part of the mesentery, which had long been quite cold, before the microscope, and still found the blood in some of the capillary vessels moving freely. I have no doubt that the blood continues to move in the capillaries of a full-grown rabbit, (the temperature of which will sink much more slowly, and the abdomen has not been laid open,) for several hours after death. This at the same time accounts for the supply of fluids to secreting surfaces, and for a certain power of the nervous system remaining after death, and when the impetus of the blood has wholly ceased, except as far as it depends on mere elasticity and the action of very small vessels.

We are now to inquire whether the nervous power is capable of occasioning a disengagement of caloric from the blood after the sensorial power has ceased to exist. It appears from experiments already laid before the reader, or referred to, that this is a function of the nervous power. But it seems so immediately connected with the existence of the circulation, and is so generally proportioned to its vigour, that we cannot, I think, adopt a better means of answering the present question, than by ascertaining whether supporting the circulation by artificial respiration after

death maintains a higher temperature than is observed in the dead animal left undisturbed. On this subject there has been great difference of opinion. The following experiments seem to point out how this difference may have arisen, on the supposition that all the experiments which have been made on the subject are correct, which we have every reason to believe them to be.

Exp. 74. Two rabbits of the same size were killed in the usual way, the temperature of the air being 61°, that of both rabbits 104°. The lungs of one were inflated six times, those of the other from twenty-six to thirty times, in a minute. The temperature of the first in half an hour was 102.25°, in an hour 100°; the temperature of the second at the end of half an hour was 101.5°, at the end of an hour 98°.

It is evident that all the air thrown into the lungs, beyond what is necessary to effect the proper change in the blood, must tend to reduce the temperature in proportion as that of the air is less than that of the animal. The living animal receives but little air into the lungs in one inspiration. It is impossible in the dead animal to throw in the quantity which the blood still demands, and no more.

The following experiments, in which Mr. Sheppard was so good as to assist me, as indeed he did in all the experiments which I made on this part of the subject, strikingly illustrate these observations.

Exp. 75. Two rabbits were chosen of the same size, and each of the temperature of 102.5°. They were killed in the usual way, in the temperature of 65°; one was left undisturbed. In the other, the lungs were inflated about thirty times in a minute. In half an hour the temperature of the undisturbed rabbit was 98.75°, while that of the other was only 98.5°. In the last the lungs were then inflated only about twelve times in a minute. In half an hour its temperature was 96°, so that it had lost 2.5°, while that of the other, left undisturbed, had in the same time sunk to 95.25°, so that it had lost 3.5°.

Exp. 76. Two rabbits were killed in a temperature of 61.5°.

The temperature of the one was 106°, of the other 103°. The lungs of the first were inflated twelve times in the minute, the other was left undisturbed. In half an hour the first had lost 3.5°, its temperature being 102.5°. The other in the same time had lost 4°, its temperature being 99°. The first, being of the highest temperature, would have cooled fastest had both been undisturbed, although probably not in a sensible degree. I may here observe, that it happened in the course of such experiments as those which I am relating, that the temperature of the room varied, but as the experiment was always made on both rabbits at the same time, and placed together, this could not influence the result, and is therefore unnoticed. The lungs of the first of the above rabbits were now inflated at the rate of from twenty-six to thirty times in a minute. At the end of half an hour its temperature was 98°, that of the other at the same time being 94.5°, so that each had now cooled 4.5°, the effect on the temperature of the inflation of the lungs being here sufficient to counteract the cooling effect of the rapid change of air, and no more. In one experiment of this kind, in which the lungs were inflated only a few times in a minute, the temperature was found to have risen nearly 1° between two of the examinations.

While the author was making experiments on this subject in Worcester, Dr. Hastings was, without his knowledge, making similar experiments at Edinburgh. He showed the author the detail of several, which prove that throwing air into the lungs of the dead rabbit about fifteen times in a minute, occasions it to cool more slowly. In one of his experiments, the rabbit, in which artificial breathing was performed, cooled only 4°, while that which was left undisturbed cooled 7.5°. This was the greatest difference he observed. He frequently saw the thermometer rise a little in those animals in which the lungs were inflated after death. In those in which they were not inflated, the cooling was always uniform.

There appears to be no doubt, from the preceding experiments, that when the lungs are not inflated so frequently as to

constitute a powerfully cooling process, their inflation maintains the temperature of the animal after what we call death.

My next object was to ascertain how far the temperature after what we call death is influenced by the destruction of the brain and spinal marrow.

Exp. 77. Two rabbits of the same size, whose temperature was 98°, were killed in the usual way. In one, immediately after death, the brain and spinal marrow were destroyed by introducing, through a hole in the cranium, a wire of nearly the same diameter with the cavity of the spine, repeatedly pushing it on to the end of this cavity, and then moving it about for some time in the cavity of the head. The other rabbit was left entire. A hole was made about the centre of the abdominal muscles in each, to admit of a thermometer being introduced into the cavity of the abdomen. They were placed near each other in a temperature of 50°. During the first twenty minutes each lost exactly 4°, and they both lost, during the succeeding three-quarters of an hour, just 2° during each quarter. Something, which we could not ascertain, accelerated the rate of cooling during the next quarter, and so exactly did it correspond in both rabbits, that each lost during this quarter 2.5°. After this their temperature diminished more slowly, and still more so, of course, as it approached more nearly to that of the air, but still in both it was found to correspond. At the end of a hundred and ten minutes the temperature of both rabbits was 84°.

Exp. 78. The foregoing experiment was repeated, with the difference that in both rabbits the lungs were inflated; but we could not perceive that the one rabbit cooled faster than the other.

Exp. 79. Two rabbits, of the same size and temperature, being killed in the usual way, in the one the brain and spinal marrow were wholly removed, the other being left entire. In both, the lungs were inflated. We could not perceive that the one cooled faster than the other.

I was particularly careful in repeating these experiments, be-

cause they appear at first view to contradict the inferences afforded by some of those which precede them. On a closer attention, however, the different results will be found perfectly consistent.

It appears from the foregoing experiments, that after the destruction of the sensorial, the nervous power is still capable of performing all its functions, except that it can no longer give evidence of conveying impressions to the sensorium, the necessary consequence of the destruction of the sensorial power. On comparing these experiments, however, a considerable difficulty presents itself. We have seen it ascertained by those on digestion, that the brain and spinal marrow retain sufficient power after visible death to form secreted fluids. Yet it would appear, from the experiments on temperature, that the influence of the brain and spinal marrow has no effect under the same circumstances in maintaining the temperature, although it is evident that the system still retains this power, and from former experiments, that this power depends on the state of the nervous system. The secreted fluids are no longer formed, if the influence of the brain be withdrawn; the maintenance of the temperature takes place in the same way, whether the influence of both the brain and spinal marrow be withdrawn or not.

A well-known fact removes the difficulty. Although we have reason to believe, from every observation on the subject, that the brain and spinal marrow are the only sources of nervous power; yet it is evident that a certain portion of this power remains in the nerves when separated from these organs, as appears from the contractions excited in the muscles by irritating their nerves under such circumstances. The muscle will thus be made to contract as long as any power remains in the nerve, but this being once exhausted, the nerve has no means of renewing it. Now the first nervous power which is employed in the stomach after death, is of course that already in its nerves. In proportion as this is exhausted, the brain and spinal marrow are called upon for a further supply. It is evident that they cannot be long so called upon, because there cannot long be any supply of proper

fluids. If then, instead of the nerves which belong to the stomach, the whole nerves of the sanguiferous system terminated in this organ, there is reason to believe that the supply of fluids which takes place after death would never be sufficient to exhaust the nervous power already in its nerves; and consequently, that in that case it would never receive any part of this power formed after death: and the same degree of digestion would take place after death, whether the influence of the brain remained or not.

Now this is precisely what necessarily happens with respect to the temperature after death. As long as we can by artificial respiration occasion such a change in the blood as elicits the power of the nervous system, the blood draws it from all the nerves of the sanguiferous system, and it does not appear that we can support this change long enough to exhaust the nervous power already in the nerves, and occasion any farther demand for it. It appears from the experiments above related, that the greatest disengagement of caloric, which can be effected after death, is but very inconsiderable. Hence the result is the same, whether the brain and spinal marrow exist or not. There is already in the nerves more nervous power than the blood can use.

The maintenance of temperature occasioned by inflating the lungs after death being so small, may arise from our being able but very imperfectly to imitate natural respiration. It is true that we can imitate it sufficiently to give the arterial colour to the blood, but it will appear from what I shall have occasion to say, that the maintenance of temperature is not essentially connected with the change of colour. It is evidently impossible to proportion the quantity of air thrown in, to the demand for it, which is constantly becoming less, so that we are either supplying too much or too little. In the former case, the superfluous quantity can, as far as relates to the temperature, have no other effect but that of reducing it, as happened in the above experiments; but although we could supply air in the due proportion, we should still be very far from being able to imitate natural respiration,

from which artificial respiration, among other things, differs, in the increased pressure to which the lungs are subjected in the latter, in which the ribs and diaphragm are moved by the force of the injected air; whereas in natural respiration the ribs and diaphragm being moved by their muscles, the lungs are subjected to no pressure in addition to that of the atmosphere. But the great diminution of nervous influence in artificial respiration is the most essential difference between it and natural breathing. For reasons that will soon appear, it would be very desirable, although attended with considerable trouble, accurately to ascertain the effects of passing the galvanic influence through the lungs while artificial respiration is performed.[1]

A very decisive experiment by Sir B. Brodie, to whose labours on the subject of animal temperature we are so much indebted, related in an addition to the Croonian Lecture above referred to, proves that the change of oxygen gas into carbonic acid gas takes place when the lungs are inflated after decapitation.

Thus it appears, from the experiments related in this chapter, that the nervous as well as the muscular power is capable of all its functions after the sensorial power is withdrawn.

The next question which presents itself is, whence does it arise that, notwithstanding the independence of the two first of these powers, they never long survive the sensorial power?

Here a difficulty presents itself, on which M. le Gallois makes the following observations:—

"Il est donc certain que la vie du tronc n'a son principe immédiat ni dans le cerveau, ni dans aucun des viscères de la poitrine et de l'abdomen; mais il ne l'est pas moins que tous ces viscères sont indispensables à son entretien. Or, en considérant sous quel rapport ils le sont, les faits énconcés plus haut prouvent évidemment que, quant au cerveau, les phénomènes mécaniques de la réspiration, c'est-à-dire, les mouvemens par

[1] See what is said of suspended animation in the third part of this Inquiry.

lesquels l'animal fait entrer l'air dans ses poumons, dépendent immédiatement de ce viscère. Ainsi, c'est principalement en tant que l'entretien de la vie dépend de la réspiration, qu'il dépend du cerveau; ce qui donne lieu à une grande difficulté. Les nerfs diaphragmatiques, et tous les autres nerfs des muscles qui servent aux phénomènes mécaniques de la réspiration, prennent naissance dans la moelle épinière, de la même manière que ceux de tous les autres muscles du tronc. Comment se fait-il donc qu'après la décapitation, les seuls mouvemens inspiratoires soient anéantis, et que les autres subsistent? C'est là, à mon sens, un des grands mystères de la puissance nerveuse; mystère qui sera dévoilé tôt ou tard, et dont la découverte jettera la plus vive lumière sur le mécanisme des fonctions de cette merveilleuse puissance."

The difficulty seems to me to arise from respiration being regarded as a function depending wholly on a combination of the nervous and muscular powers; whereas the sensorial power appears evidently to share in it.

The muscles of respiration are, in the strictest sense of the word, muscles of voluntary motion; we can at pleasure interrupt, renew, accelerate, or retard their action; and, if we cannot wholly prevent it, it is for the same reason that we cannot prevent the action of the muscles of the arm, when fire is applied to the fingers. The pain occasioned by the interruption of a supply of air to the lungs is greater than can be voluntarily borne. Respiration continues in sleep for the same reason that we turn ourselves in sleep when our posture becomes uneasy. It continues in apoplexy for the same reason that the patient generally moves his limbs if they are violently irritated.

If respiration continues in apoplexy when no irritation of the limbs, however powerful, can excite the patient to move them, it arises from the interruption of a supply of air to the lungs producing a greater degree of irritation than any other means we can employ. We have heard of the hand voluntarily held in the fire, but we know of no instance where the breathing has been voluntarily discontinued till the lungs were injured. As the insen-

sibility increases in apoplexy, the breathing becomes less frequent; and when it becomes such that no means can longer excite any degree of feeling, the breathing ceases.

By a certain sensation, a wish is excited to expand the chest. This is an act of the sensorium. Till this act take place, the nervous as well as the muscular power, by which its expansion is effected, is inert; it is in vain that these powers remain, if the power which calls them into action be lost.

Thus two of the sensorial powers, sensation and volition, become essential to the preservation of life in the more perfect animal.

Is it said that the motions of respiration must be involuntary, because we are in general unconscious of them? But do we not become more or less so of all habitual acts of volition? "If I did so, I did it unconsciously," is a common expression. If we stop a person who is walking, he cannot tell which leg he last moved, or a person who is playing on an instrument, he cannot tell which fingers he last employed; yet all such acts are strictly acts of volition. If we are reminded of them, we can always interrupt, renew, retard, or accelerate them at pleasure. We have no difficulty in perceiving and changing in any way we please the motions of respiration, when we choose to attend to them; but as there is no other act of volition so habitual, there is none so apt to escape our attention.

The foregoing explanation of the manner in which the removal of the brain puts a stop to respiration will be readily admitted, when the reader turns his attention to the part of the brain to which impressions from the lungs are conveyed. It is evidently to the part where the eighth pair of nerves, which supplies them, originates, and from which the spinal marrow proceeds. These nerves are no ways connected with the muscles of respiration, they only convey to the sensorium the sensations excited in the lungs; and to the lungs the influence on which their assimilating and secreting powers depend; and it appears from the experiments in which M. le Gallois removed the brain by slices, that respiration continued till he removed this part of it, and then instantly

ceased. In these experiments, the power of the muscles of respiration and the nervous power which excites them still remain, as may be easily ascertained by stimulants properly applied to the spinal marrow. It is the sensation which excites to inspire, that is the influence of the sensorial power which is withdrawn.

We cannot, perhaps, have a better instance of the distinct operation of the sensorial, nervous, and muscular powers, than in the case before us, although they all here conduce to the same end. We may destroy any one of them, and leave the others unimpaired. The destruction of the sensation by which we will to inspire, the reader has just seen, does not destroy the nervous or muscular power employed in respiration. By means applied to the muscles of respiration, we may destroy their mechanism without depriving any part of the spinal marrow of its power, or at all impairing the sensation which makes us will to inspire; and we may destroy the nervous influence which excites these muscles by destroying a certain part of the spinal marrow, while they, as may be ascertained by the application of stimulants, perfectly retain their vigour, and the sensation which excites the wish to inspire, though, as in the last case, useless, remains unimpaired; nay, if any two of these powers be destroyed, they leave the remaining power unimpaired. The destruction of the muscles of inspiration, and of the nervous influence which excites them, does not destroy the sensation by which we will to inspire; nor does the destruction of this sensation and the nervous influence at all impair the power of the muscles; and we may destroy the sensation in question, and the power of the muscles, without impairing the nervous influence which excites them. So far from true is the position of M. le Gallois, that the power on which all the motions of inspiration depend, resides in the medulla oblongata. This is only the part in which resides the cause of the sensation which calls the other powers of respiration into action.

Much has been written by Whytt and many other physiologists, respecting the cause of the first inspiration. I cannot help thinking that the difficulty vanishes, when we regard the muscles of inspiration as merely muscles of voluntary motion.

The young animal throws them into action to remove a painful sensation occasioned by the want of that change in the blood, which is produced by the influence of the air in the lungs; a process necessary to the existence of the animal as soon as its connexion with the mother ceases, and which can only be effected by expanding the chest, and thus receiving air into the lungs. It seems to be expanded for the first time, precisely for the same reason that the fœtus changes its position for the first time by acting with the muscles of the trunk and limbs. In both cases he endeavours to remove an uneasy sensation, and nature has given him the power to remove it by calling into action certain muscles subjected to the will. The first act of deglutition, if it does not occur in the fœtal state, appears to be an act of precisely the same nature with the first inspiration. In both, a certain set of muscles of voluntary motion is thrown into action, to satisfy a craving which had no existence in that state.

It may be objected to this view of the first inspiration, that the animal often breathes before a ligature is thrown round the vessels which connect it with the mother; but we have no reason to believe, that the secondary change, effected in the blood of the fœtus by the vicinity of the maternal blood in the after-birth, although this gives it the florid colour, as may be seen by opening the vessels, is sufficient for the functions of the perfect animal. One of these functions, which, as the reader has seen, is intimately connected with the change effected on the blood by the air, the maintenance of the temperature, it is evident, is immediately after birth required to be in a state of much greater activity than in the fœtus, which is surrounded by a medium of its own temperature.

Why the sensorial powers are the first which fail in dying will be considered in the next chapter; it is sufficient at present to refer the reader to the experiments which prove that all the nervous and muscular powers, with the exception of those cases in which the nervous system is so impressed as immediately to destroy all the functions, survive them.

A necessary consequence of the sensorial powers being the first which fail, is, that respiration is the first vital function which ceases, being the only one to which the sensorial powers are necessary.

When respiration ceases, the change in the blood effected by this function no longer taking place, a large proportion of the pulmonary vessels lose their proper stimulant, red blood, and feel more directly perhaps the debilitating influence of black blood. Their functions, therefore, begin to fail. In proportion as this happens, the blood accumulates in the lungs. The right side of the heart consequently experiences an increased difficulty in emptying itself, and the due supply of blood to the left side fails. By the operation of these causes, both sides of the heart, in warm-blooded animals, soon lose their power after respiration ceases. The arteries, under such circumstances, it is evident, cannot long supply fluids proper for the purposes of secretion; the nervous and muscular solids, therefore, deviate from the state necessary for the functions of life, which at length cease in every part.

Such appears to be the order in which the functions always, with the exception just mentioned, cease in death, whether it be occasioned by injury of the sanguiferous or nervous system, or both.

The acute and indefatigable Bichat has been at great pains to ascertain the effects of black blood on the lungs and other organs. To his experiments on this subject I refer the reader. There are but few parts of his physiological works, however, which can be confidently referred to. In general, he has allowed his reasonings to go beyond the evidence afforded by his observations and experiments. I shall take this opportunity of making a few remarks relating to the principal points in which I have differed from him. He was unacquainted with the fact, that the spinal marrow performs its functions independently of the brain,[1] and there-

[1] The independence of the spinal marrow on the brain, as far as relates to its power over the muscles of voluntary motion, appears from the experiments of M. le Gallois; and as far as relates to secretion, from expe-

fore did not see the difficulty respecting respiration stated by M. le Gallois, but seems to think that the division of the spinal marrow near the head occasions death, by preventing the nervous influence of the brain from reaching the intercostal muscles and diaphragm. The want of this knowledge leads him into inaccuracies, both in his observations on death, and other passages: which are increased by his not being aware of the different nature of the dependence the sensorial and nervous powers have on each other. He is led into more obvious errors, as far as I am capable of judging, in various parts of his works, particularly in those which relate to the passions and the death of the brain, by his not knowing that the heart and blood-vessels may be directly influenced, and even their power directly destroyed, by agents acting either on the brain or spinal marrow; by his not being aware of the dependence of the process of secretion on the brain and spinal marrow, and by his supposing that the ganglions are capable of preparing nervous influence independently of these organs; a supposition which the reader has seen contradicted by direct experiment, and which Bichat does not attempt to support by any observation or experiment directly bearing on the point.

These circumstances have even led him into the most striking inconsistencies in his great division of the functions into organic and animal. If the experiments which have been laid before the reader be correct, the sensorial with the cerebral part of the nervous functions constitute the animal, and the ganglionic the organic life, the muscular function being associated with both. To this it may be objected, that plants and the less perfect animals have no ganglionic system. Wherever secretion is performed, a power resembling the ganglionic must exist. In order that a being, possessed of the muscular and secreting systems alone, may be in perfect vigour, it is only necessary that respiration should be performed, as circulation is, by powers of involuntary motion. A being so formed, though possessed of all the powers of life,

riments laid before the reader in the second section of the seventh chapter of this Inquiry.

would be wholly unconnected with the external world, except as far as food and the influence of air and light are necessary to its existence; all other intercourse with that world depending on the sensorial functions. Such is the life of vegetables, and we have reason to believe that that of the lowest class of animals differs from it in little else than degree.[1] An animal of this class approaches, as nearly as facts will allow us to suppose, to one possessing merely organic life, according to Bichat's definition of it; yet, in the second section of his sixth article, he maintains, that everything relative to the passions belongs to the organic life; an inconsistency, itself sufficient to prove a radical defect in his system. Can the passions belong alone to that life in which they never can be excited, in which they never could operate? Even according to Bichat's definition of organic life, it is common to the animal and vegetable world.

The various functions necessary to the preservation of life in the more perfect animals have now been considered. It appears from the experiments and observations which have been laid before the reader, that as the muscular is independent of the nervous power, yet influenced by it; the nervous is independent of the sensorial power, yet in like manner influenced by it; for we have found that the sensorial can be withdrawn without destroying any of the functions of the nervous power, as the nervous can be withdrawn without destroying the muscular power; yet in the entire animal, as the muscular obeys the nervous, the nervous obeys the sensorial power.

[1] I do not mean that the change effected on the air by plants is of the same nature with that effected by animals, or that they possess a circulation similar to that of animals; but we know that air is necessary to their existence, that some change in it is effected by them, and that in their vessels or canals there is a continual motion of their fluids.

CHAPTER XIX.

On the Nature of the Vital Powers.

The first observation which strikes us, in comparing the sensorial and nervous functions, is, that the latter bear a striking, the former no analogy to the operations of inanimate nature. The act of secretion, the processes of assimilation, and the means by which animal temperature is maintained, are analogous to the processes of the laboratory; and the transmission of impressions through the nerves, both to chemical and mechanical processes; while the excitement of the muscular fibre is the ready effect of many inanimate agents. But what analogy can we detect between the functions of the sensorial power, sensation and volition for example, and the effect of inanimate agents?

We are here in a new world, and at once perceive that it is in vain to look for the analogies which necessarily suggest themselves on reviewing the phenomena of the nervous system. It seems to require but a moment's reflection to teach every sober and unprepossessed understanding, that, in our study of the sensorial power, we must be satisfied with observing and arranging its phenomena, without attempting to refer them to any more general principle.

On a review of all that has been laid before the reader, it is evident that the nervous and muscular powers are, on the one hand, the direct means of maintaining the life of the animal; and on the other, of connecting it with the external world; the former receiving impressions from that world, and through the latter communicating impressions to it. All the functions of both powers bear a strong analogy to the properties of the world with which they are thus associated; and we have reason to believe

that all these functions, as is evidently the case with many of them, are the results of inanimate agents acting on vital parts.

As vital properties do not differ from the properties of inanimate nature, in degree or by any other modification, but have nothing in common with them, it follows that when living bodies affect each other only by their vital properties, the result must be such as bears no analogy to any of the properties of inanimate nature; and, consequently, that in all processes which have any such analogy, one of the agents must operate by the properties of inanimate matter.

In the animal body itself, the nervous system alone appears to be the connecting link between the sensorium and inanimate matter. It consists of living parts capable of acting in concert with that matter, receiving impressions from it, and independently of the intervention of the muscular system impressing it; for there can be no stronger analogy than that which subsists between the secreting processes, effected by the nervous power in living surfaces, and the chemical processes of inanimate matter; and if an inanimate agent be employed in the former processes, its supply and application must be regulated by the vital powers of the nervous system. Whether this agent be a distinct being, or only a peculiar state of the constituent parts of bodies, is not the question. All the essential inferences are in either case the same. The phenomena of electric animals are here in point. We see their nervous system collecting or forming and applying, even according to the dictates of the will, an agent that operates in inanimate nature.

With respect to the sensorial functions, they have only an indirect effect in maintaining animal life, and are excited by no impressions but those communicated through the nervous system. They are therefore the results of vital parts acting on each other, and the nature of these results proves that in their production the vital properties of the parts concerned are alone employed, because in them there is not a trace of any of the properties of inanimate nature. All such analogies have disappeared. Hence

it is that they are the first functions which cease when the vital powers fail. In the nervous and muscular functions, an inanimate agent, as will soon more clearly appear, excites the languid powers of life. In the sensorial functions, the functional power and the agent which excites it, being equally vital powers, fail together.

When the nature of the sensorial functions is kept in view, we cannot be surprised that the attempts to refer them to a more general principle should have proved so futile. To what other principle shall we refer the effects of the vital parts of animals on each other, when it is in animals alone that such parts ever influence each other? Even in vegetable life we find nothing analogous to the sensorial functions. All its processes bear the same analogy to the properties of inanimate nature which we observe in the functions of the nervous and muscular system of animals, and are therefore the results of inanimate agents acting on living parts. Much less can we look for any analogies of this kind in inanimate nature itself. Such fancied analogies may please in the creations of the poet, but by the philosopher they are justly rejected. While we are charmed with the flights of Lucretius, we see only the perversion of philosophy in the reasonings of Hartley.

While these observations point out the necessity of limiting our study of the sensorial functions to a careful observation and arrangement of their phenomena, they encourage us to inquire into the nature of the functions of the nervous power, that is, to endeavour to refer them to some more general law whose phenomena, however modified, are not wholly changed by one of the agents in these functions being a vital part. The power which operates in many other instances may be the means of exciting the muscles, of effecting the formation of the secreted fluids, of maintaining the various assimilating processes, and the due temperature of the warm-blooded animal.

Such is the train of reasoning which seems unavoidably to lead to the conclusion, that the nervous power is an inanimate agent. It appeared to me that it might not be impossible to submit this

conclusion to the test of experiment. Nobody will hesitate to admit that the vital functions depend on the organisation of the parts in which they reside. If the organisation of the muscular fibre be deranged, its function ceases. The same is true of the functions of the brain, spinal marrow, &c.

Whether the function depends merely on that organisation, or on something superadded to the organ, the presence of which depends on the organisation, we know not. The fact is, that when the peculiar organisation is disturbed, the function is lost. A power which can exist independently of the peculiar organisation of the part in which it resides, is not a vital power. It must be classed with those powers which are capable of existing in inorganised bodies.

The nervous power, whatever it be, is evidently conveyed by the nerves. If it be a vital power, its existence depends on the organisation of the part in which it exists. Its propagation along the nerve, therefore, depends on the peculiar organisation of the nerve, and cannot be performed by any other part. If the nervous power can be conveyed by other parts, it is not a vital power, but one that may reside in inorganised bodies.

With a view to determine this question, with the assistance of Dr. Hastings and Mr. Sheppard, I made many experiments on nerves directly proceeding from the spinal marrow, but could obtain no evidence of the nervous power having been conveyed by any part but the nerve itself. Circumstances which I shall soon have occasion to mention, led me to make the experiment on the ganglionic nerves, in which Mr. Cutler was so obliging as to assist him.

Exp. 79. The reader has seen, that removing a part of the eighth pair of nerves prevents the formation of the gastric juice, the fluid secreted in the stomach, which at length fails altogether, being no longer capable of acting on the food. At my request, Mr. Cutler divided, without otherwise disturbing, both of the eighth pair of nerves in the neck of a rabbit, which had been allowed, immediately before the operation, to eat as much as it

chose after a fast of many hours. The divided ends of both nerves retracted, so as to cause a separation between them of at least a quarter of an inch on both sides. The animal died in about eight hours, and although the digestion was much deranged, such a change was found to have taken place in the food, as proved that part of the nervous power conveyed by the eighth pair of nerves had reached the stomach. The result was such as left no doubt in the mind of any of those who witnessed it, among whom was Sir Benjamin Brodie.

M. Breschet, Dr. Milne Edwards, and M. Vavasseur of Paris,[1] have since repeated this experiment on a large scale, and confirmed its result.

It seems, at first view, surprising that the influence of the nervous system should pass so readily by the ganglionic nerves after their division, since it appears from the experiments above referred to, made on the spinal nerves, that this does not happen with respect to these nerves. But the different circumstances in which the two sets of nerves are placed seem readily to explain the difficulty. We know that the power of secreting surfaces is increased for the time, if they retain their healthy state, by any cause which occasions a greater than usual determination of blood to them. The presence of this fluid in such surfaces, therefore, solicits towards them a corresponding supply of the influence of the nervous power. Thus there is a cause soliciting a flow of this influence to the extremities of the ganglionic nerves, which has no existence in the case of the cerebral and spinal nerves. There is nothing in the muscular fibres to solicit this influence. They are passive till it is applied to them.

Thus that the nervous power must be an agent which belongs to inanimate nature appears from direct experiment, as well as

[1] De l'Influence du Système Nerveux sur la Digestion Stomachale, par MM. Breschet, D.M.P., chef des Travaux Anatomiques de la Faculté de Médecine de Paris, etc. H. Milne Edwards, D.M.P., et Vavasseur, D.M.P. (Mémoire lu à la Société Philomatique, le 2 Août, 1823.) Extrait des *Archives Générales de Médecine, Août,* 1823.

the most obvious train of reasoning. There is still another evidence of the same fact, which alone perhaps would be sufficient. In its most simple function we can substitute for the nervous power a variety of inanimate agents. The muscles can be excited to contract in precisely the same way, as far as we can see, both by mechanical and chemical agents, as by the nervous power. As soon as it was ascertained that the power of the muscular fibre resides in itself, and that the nervous power acts only as a stimulant to this fibre, the question which has just engaged the reader's attention appears to have been determined. If there be nothing in common between vital properties and those of inanimate agents, how comes it that a vital power here performs a function apparently as well performed by those agents? If the nervous power imparts no power to the muscular fibre, its effect on it is precisely the same with that of other stimulants.

In considering the inference from this power having passed the space between the divided ends of the nerve, the reader must recollect that it is not merely capable of passing along the nerve from the brain and spinal marrow, but of remaining in the nerve when those organs no longer exist.

There is no better proof of having arrived at truth, than that arguments drawn from dissimilar sources conspire in confirming the result of our inquiry. Here a simple train of reasoning, unequivocal experiment, and every day's experience of the effects of stimulants on the muscular fibre, tell us that the nervous power is an agent of inanimate nature.

It only remains to inquire whether this agent is peculiar to the animal body, or the same which operates in the production of other phenomena. This is a question which can be determined by experiment alone.

When we look throughout inanimate nature for an agent capable of the phenomena of the nervous power, the most subtle, electricity, naturally suggests itself; and when voltaic electricity and its signal influence on the muscular system was discovered,

a material step, it was imagined, had been made towards ascertaining the nature of the nervous power. On more mature reflection, however, it was admitted that to ascertain that voltaic electricity is capable of exciting the muscular fibre, a property possessed in common with so many other bodies, is to go but a very short way towards establishing its agency in the phenomena of the nervous system, and of late the opinion appears to have been abandoned. If the nervous power and voltaic electricity be really the same agent, the latter must be capable of the more complicated, as well as the more simple, functions of that power.

On comparing the properties of this form of electricity with the phenomena of the nervous power, the analogy between them seemed to me to warrant the investigation thus suggested.

The reader has seen that by the removal of part of the eighth pair of nerves, the power of digestion, and consequently the formation of gastric juice, is wholly lost, and the structure of the lungs as well as their secreting power deranged. This appeared to offer an excellent opportunity of ascertaining how far voltaic electricity is capable of effecting the more complicated functions of the nervous system. It is not difficult, by coating the lower part of the divided nerves with tinfoil, and applying a plate of metal to the skin over the stomach and lungs, to expose these organs, by means of a voltaic trough, to any degree of electricity which may be judged proper. I explained my views to Dr. Hastings, who, at my request, was so good as to make the following experiment.

Exp. 80. The hair was shaved off the skin over the stomach of a young rabbit, and a shilling bound on it. Part of the eighth pair of nerves was then removed, and about a quarter of an inch of the lower remaining portion of each nerve coated with tinfoil. The tinfoil and shilling were connected with the opposite ends of a voltaic trough, containing fifty-two four-inch plates of zinc and copper, the intervals being filled with muriatic acid and water, in the proportion of one of acid to seven of water. The

voltaic influence produced strong contraction of the muscles, particularly of the fore limbs.

For five hours the animal continued quite free from the symptoms which follow the division of the eighth pair of nerves in rabbits. It had neither vomited nor been distressed with difficulty of breathing. It had not eaten anything after the nerves were divided. At this time the power of the trough became much weaker, so that it produced no visible effect on the muscles. The respiration now began to be disordered, and soon became very difficult. Acid was put into the trough till the galvanic power became as great as at first, and the animal soon breathed with greater freedom. The voltaic process was several times discontinued and renewed, so that we repeatedly saw the extreme dyspnœa return on discontinuing, and abate on renewing it. The animal died in six hours after the division of the nerves.

On examination, no food was found in the passage to the stomach. This organ was not larger than usual. The food had undergone a considerable change. The appearance and smell of the parsley were gone. The smell was that peculiar to the rabbit's stomach while digestion is going on. Both Dr. Hastings and myself, who had been much accustomed to examine the stomach of rabbits under various circumstances, thought that digestion was nearly as perfect as it would have been in the same time in a healthy rabbit. This rabbit had not eaten anything for twelve hours till within three hours of the experiment; it was then very hungry, and allowed to eat as much parsley as it chose.

The membrane of the windpipe was of its natural colour, and there was no fluid in it. The ramifications of the air-tubes in the left lung were quite free from frothy mucus. There was some fluid in the right lung, though it did not appear much gorged; there was one dark spot on it. The lungs collapsed imperfectly on opening the chest. I requested Dr. Hastings to make the experiment in the following manner:—

Exp. 81. Two full grown rabbits were kept without food for twelve hours; within half an hour of the experiment, they ate as much parsley as rabbits usually do at one time. After the hair

was shaved off the region of the stomach in one of them, and a shilling bound upon it, part of the eighth pair of nerves was removed, and the lower remaining portion of the nerve coated with tinfoil, to the extent of half an inch. Difficulty of breathing was evident immediately after the operation. It was an hour after it before the animal was brought properly under the voltaic influence; till then the respiration was very much oppressed; it soon improved on keeping up a regular and gentle twitching of the muscles of the chest and fore-legs. In this instance a less voltaic power was employed, the third, half, or whole of the trough being used, according to its effects. A gentle twitching of the fore-legs was regarded as the measure of a due degree of voltaic power. The animal made a croaking noise in respiration, the cause of which will presently be explained. The breathing always began to get worse when the galvanic power became too weak to produce any twitching in the fore-legs.

About twelve hours after the operation, each rabbit ate the same quantity of parsley. Fourteen hours after the nerves had been divided, the galvanic influence had become too feeble, and the animal made one attempt to vomit. The power of the trough was increased, and no further attempt to vomit took place. After this, however, the breathing continued more or less oppressed. It died in seventeen hours after the removal of part of the nerves. The other rabbit, which had been left undisturbed, was killed at the same time.

The stomachs of both were laid open. That of the rabbit, in which part of the nerves had been removed, was not more distended than the stomach of the other. The food in it had the appearance which it has in a healthy stomach while digestion is going on. The only differences between the contents of the two stomachs were the following:—The food which the healthy rabbit had taken during the experiment, was found in the cardiac portion of the stomach, and digestion was going on rapidly in it, while that which the other had taken at the same time, was still in the passage to the stomach, and consequently unchanged. The position of this animal was unfavourable to the food's reach

ing the stomach. There was about a quarter of an inch of the passage between the food and the stomach quite empty. The contents of the middle portion of the two stomachs could not be distinguished from each other; those in the pyloric end only differed in being of a firmer existence in the healthy rabbit. In both, the contents of the pyloric end of the stomach were most digested, and in both the food had equally lost the appearance and smell of parsley, and acquired the smell peculiar to the stomach of the rabbit while digestion is going on. The food in the duodenum was equally digested in both. The reader has seen, that after part of the eighth pair of nerves is removed, parsley will remain in the stomach of a rabbit wholly unchanged for six-and-twenty hours. Neither Dr. Hastings nor myself could, from anything observed in the stomach of the rabbit which was subjected to the voltaic influence in this experiment, have doubted its being the stomach of a healthy rabbit.

If we compare the foregoing experiment with that in which only one of the eighth pair of nerves was divided, we find the difference of result very striking. In the latter, parsley remained in the stomach unchanged for nearly two days. In the former, although a part had been removed from both nerves, the whole food contained in the stomach, notwithstanding it had lain in it a comparatively short time, was nearly as much changed as in the stomach of a healthy rabbit.

The membrane of the windpipe appeared of a very deep-red colour, but there was not much fluid in it. The lungs did not collapse on opening the thorax, the air-cells being full of a frothy and bloody serum. The lungs were externally of an uniform dark-red colour. The heart was a little increased in size, and highly vascular. Throughout the whole of the body there was a general increase of vascularity. This, as appeared from what was observed in other cases, was the effect of the voltaic influence.

The cause of the peculiar difficulty of breathing in this experiment, and of the croaking noise, was, that the nerves of the

upper part of the windpipe were injured by being stretched, in order to divide the eighth pair as near to them as possible. The reader will see from what is said above, that difficulty of breathing and a croaking noise are the effects of injuring those nerves. They, it is evident, were out of the voltaic circle.

We learn from the state of the lungs after the division of the eighth pair of nerves, why an animal cannot be long preserved by artificial respiration after the brain is removed. M. le Gallois, who was perhaps inferior to no physiologist in accuracy of observation, remarks, that artificial respiration produces the same loaded state of the lungs. This is the effect of the nervous power being withdrawn. The artificial respiration, by preserving the life of the animal, only gives time for the effect to take place.

In the foregoing experiments, the voltaic influence had been directed chiefly to the region of the stomach. In order to ascertain the effect of directing it more particularly to the lungs, I requested Dr. Hastings to make the experiment in the following manner.

Exp. 82. A full-grown rabbit was kept without food for twelve hours. Within two hours of the experiment, it was allowed to eat as much parsley as it chose. The hair being then shaved off the skin of the chest, it was covered with tinfoil, but not so low as the pit of the stomach. Part of the eighth pair of nerves was now removed lower down in the neck than in the last experiment, and the lower remaining portion of both nerves coated with tinfoil for about a quarter of an inch. The nerves of the upper part of the windpipe were not disturbed, and no croaking ensued. It was an hour after the operation before the voltaic influence was so applied as to keep up a gentle twitching of the muscles of the chest and fore-legs. This effect of the voltaic electricity was kept up uniformly for five hours, during which time there was hardly any dyspnœa.

In about an hour after this, an uniform effect from the trough could not be kept up, on account of the tinfoil having been torn.

The breathing became much worse, and the tinfoil could not again be generally applied to the chest. It was kept imperfectly applied till the death of the animal, which happened in about two hours and a half after this; that is, about nine hours and a half after the division of the nerves. The animal had shown no tendency to vomit.

The lungs collapsed on opening the chest, though not so perfectly as when they are healthy: they swam in water. The inner membrane of the windpipe was redder and more vascular than usual: there was no frothy mucus in it. The air-cells, near the great division of the windpipe, were full of phlegm; but, on tracing them further, there was very little, and none at all towards the surface of the lungs, which was of a deeper red than natural, but not showing patches of red, as where a part of the eighth pair of nerves is removed without the application of voltaic electricity. The heart was much more vascular than natural. The same observation applies to every part of the thorax. The thoracic viscera, in short, were rather in a state of inflammation—an effect always produced by the long continuance of a considerable voltaic power in the part to which it is directed—than in that in which they are found after the division of the eighth pair of nerves.

The stomach was larger than natural, and the food but little altered, retaining the colour, smell, and stringiness of the parsley. There was no food in the passage to the stomach. It is evident that the stomach, in this experiment, was but little exposed to the voltaic influence. It was sufficiently so, however, to prevent the vomiting, and to occasion more change in the food than happens when a part is cut out from the nerves without the employment of the voltaic influence.

The reader may remark, that as the stomach is here found in a state intermediate between that of this organ, when part of the eighth pair of nerves is removed without the application of voltaic electricity, and when the electricity is chiefly directed to it; so the state of the lungs in Exps. 80 and 81 is intermediate between

what it was in this experiment, and what it is when part of the nerves is removed without the application of this power.

As the foregoing experiments were made on a granivorous animal, and some objections were started to them on this account, a carnivorous animal was chosen for the subject of the following experiment, which Mr. Sheppard was so kind as to perform at my request.

Exp. 83. Two small dogs, of the same size and age, were kept without food for about thirteen hours; they were then permitted to eat as much lean raw mutton as they chose. In both, the eighth pair of nerves were divided immediately after they had taken the mutton. In one of them the nerves were coated with tinfoil, as they had been in the rabbits, a three-shilling piece having been previously bound on the pit of the stomach and lower part of the thorax, after the hair had been shaved off; and voltaic electricity applied as in the foregoing experiments.

The apparatus had been so arranged, that the electric power was applied as soon as the nerves were divided.

The dog which was not exposed to it was almost immediately affected with dyspnœa, and within ten minutes with repeated efforts to vomit. The other, to which it was applied of sufficient strength to occasion a very gentle motion in the fore-legs, but not any expression of pain, breathed as free as before the division of the nerves, and never made any effort to vomit. Its application was twice discontinued for a few seconds, during which the animal breathed very laboriously, but on its reapplication the breathing immediately became free. This dog lived two hours and a quarter.

On opening the stomach after death, we found the mutton half digested. It had lost its red colour, and was reduced to a soft pulpy substance, in which there was little or no appearance of muscular fibre. That part of the mutton which lay in the pyloric end of the stomach was most digested, a proof that digestion was going on in the usual way. The vessels in some parts of the sto-

mach, and throughout the whole of the small intestines, were highly injected, giving those parts a very florid appearance. The lungs were rather redder than natural, but otherwise quite healthy, collapsing perfectly on the thorax being opened. It is of great consequence, in judging of the effects of voltaic electricity on the lungs, that the galvanic apparatus should be arranged before the commencement of the experiment, that the animal may be subjected to its influence as soon as part of the nerves is removed, and thus the difficulty of breathing wholly prevented. Another proof of this the reader will see in an experiment soon to be related.

The other dog, which was still alive at the end of four hours after the nerves had been divided, was killed at this time. The mutton, although it had been in its stomach so much longer than in the other dog, was as firm as when it was swallowed, and perfectly retained both its red colour and fibrous appearance, except that on the outside the bits seemed as if they had been dipped in boiling water: immediately below the surface they were quite red. The lungs exhibited the same appearance as those of rabbits under the same circumstances. They were so obstructed, that they collapsed very imperfectly, and their surface was covered with patches of a dark-red colour.

There was nothing in the stomach of either dog but the mutton, which was taken at the commencement of the experiment, and no part of it had been thrown back into the passage leading to the stomach in either. All present at this and the preceding experiments examined the state of the stomach and lungs, and expressed their entire satisfaction in the results.

These results were thought so improbable by many, that several gentlemen of the highest reputation as physiologists thought it proper that the experiment should be repeated. On repeating it, the result they obtained was in opposition to that which had occurred to me, the digestive process not appearing to be promoted by the voltaic influence. This led to a second repetition of the experiment, (an account of which was published in the

London Medical and Physical Journal, for May, 1820, vol. xliii. p. 385,) by Clarke Abel, M. D. F.R.S., &c.

The results obtained by Dr. Abel corresponded with those which had occurred to me, but naturally failed to convince the gentlemen who had themselves met with a different result; and when I came to London in 1820, even those who were most averse to experiments on living animals, agreed that it was necessary to set the question at rest by a public repetition of the experiment, by which all who felt any interest in it might have an opportunity of witnessing its different stages, and judging for themselves of the results. It was consequently repeated in 1821, and Sir Humphrey Davy, then President of the Royal Society, who was not more distinguished by the splendour of his discoveries, than eminent for his liberality and love of science, permitted the experiment to be made in the rooms of the Royal Institution, where free access was allowed to every one who desired it.

Among those who witnessed the result were the President of the Royal Society, Mr. Andrew Knight, and other members of the Society, and many distinguished members of the medical profession, all of whom acknowledged the accuracy of my statements respecting the effect of voltaic electricity in restoring the digestive power of the stomach.

One of the gentlemen who had assisted at the first repetition of the experiment, Sir Benjamin Brodie, whose abilities and whose candour have justly placed him so high in public estimation, and who on this occasion did me the honour of performing the operative part of the experiment, was the first to acknowledge the accuracy of those statements; and Mr. Broughton, who had publicly expressed his doubts of them, in an account of the experiment published in the ensuing number of the *Journal of Science*, in the handsomest manner acknowledged their accuracy. The results both of this and the seventy-eighth experiment were laid before the Royal Society, and published in the *Philosophical Transactions* of the following year. I cannot recur to these circumstances without expressing in the strongest terms my

sense of the candour and liberality I experienced from all with whom I had any intercourse on this occasion. To the politeness of Mr. Brande, of the Royal Institution, I was at this time, and have since been much indebted.

In the foregoing experiment the nerves were divided and treated in the same way in two rabbits of the same age, one only being subjected to the electric power, that those who witnessed the result might have an opportunity of comparing the contents of the two stomachs.

The apparatus was arranged previous to the commencement of the experiment, and one of the rabbits was subjected to the voltaic power as soon as its nerves were divided. No dyspnœa whatever, and no effort to vomit, occurred in it, while the other was affected with both to a great degree. The voltaic power was uniformly such as to keep up a gentle motion of the fore-legs, but to produce no other sensible effect. Both rabbits were killed in about six hours after the division of the nerves.

With respect to the lungs, those of the non-electrified rabbit, as in the preceding experiments, were much obstructed, and evidently changed in their structure, with patches of a dark-red colour on their surface. The lungs of that electrified appeared perfectly healthy, the galvanism not having been continued for a sufficient length of time to inflame them. The experiment having been made with a view to the state of the stomach alone, most of the gentlemen had left the room before that of the lungs was examined, which could not be done without disturbing the contents of the stomachs.

In the first stage of the experiment a difficulty presented itself, which led to a detection of a principal cause of the difference of result which had occurred in the first instance. Before it can be ascertained whether voltaic electricity is capable of restoring to the stomach the power of digestion, it is evidently necessary to deprive it of this power; for which purpose, Sir Benjamin Brodie divided the eighth pair of nerves in the neck of a rabbit, by which digestion was deranged, but not arrested in the

way in which I had seen it in my own experiments, the operative part of which had been performed by Dr. Hastings.

On minutely comparing Dr. Hastings' and Sir Benjamin's mode of operating, the only difference which the author could detect was, that Sir Benjamin only divided the nerves, while Dr. Hastings had always removed part of them. This circumstance appeared too trifling to occasion the difference of result observed; and when I, influenced by views which have already been explained, suggested that it was possible that the influence of the nervous system might pass along a divided nerve, I found none to second my opinion, which was regarded as little better than visionary. It was on this occasion that Mr. Cutler, at my request, was so good as to perform the experiment which has been related above (78,) which, as has been stated, convinced those who witnessed it, that the nervous power had passed along the nerves, although their divided ends had retracted from each other to the distance of a quarter of an inch, nothing but the moisture of the parts lying between the divided ends.

The reader will readily perceive, that in the first repetition of the experiment, the digestion having only been partially disordered by simply dividing the nerves, the effect of the galvanism could not have been so remarkable as in the original experiment.

He has seen that the experiment just referred to was repeated, and its result confirmed by M. Breschet, Dr. Milne Edwards, and M. Vavasseur of Paris. These physiologists also repeated the other experiments related in this chapter on a scale much more extensive than was attempted in this country, and on a greater variety of animals.[1] I consider myself much indebted to them for the ample confirmation they have afforded of the results just laid before the reader, and which will make any further repetition of the experiments unnecessary.

In the experiment performed at the Royal Institution, neither Sir Benjamin Brodie's nor Dr. Hastings' method was followed;

[1] *Archives Générales de Médecine, Août*, 1823.

but, at my request, after the division of the nerves, a thread having been passed through the lower portion of each close to the divided end, they were raised out of their place, and their position secured by the thread being tied round the neck of the animal. This I judged the most effectual means of preventing the influence of the brain from passing from the upper to the lower portion of the nerve.

Exp. 83. It was suggested, that, by the power of voltaic electricity, the degree of digestion which takes place after death might be increased. But we could not cause the voltaic electricity to produce any appearance of inflammation, either in the stomach or bowels, an effect which the powerful continued action of galvanism always produces in the living animal. The absence of inflammation here seems to arise from the galvanism producing little or no increased impetus of the blood in the dead animal. Thus, whatever increase of nervous influence there may be, there can be no increased supply of fluids for it to act upon, without which, it is evident, there can be no increased secretion.

The author has no doubt that the slight degree of digestion, after death, which is prevented by dividing the eighth pair of nerves, might, after the division of these nerves, be restored by galvanism. This is an inference from the experiments which have been laid before the reader. To make the experiment would be very laborious, because, for reasons mentioned above, it must be made on a very large scale, and the voltaic influence applied in each instance as long as the motion of the blood in the capillaries continues after death, probably for two or three hours.

Exp. 84. Although it is difficult to ascertain whether voltaic electricity influences the state of the stomach after death, the case is very different with respect to the lungs. In them it is easy, from the degree of patching, with precision to ascertain the state of the assimilating functions. The patches are occasioned, we have seen, by dividing the eighth pair of nerves after death. Here, however, there was no occasion for minute observation, for in nine instances, in which the eighth pair of nerves were divided

after death, and the voltaic influence was sent through the lungs for only about a quarter of an hour after death, all appearance of patching was prevented; the lungs, after the animal had lain dead about twenty hours, appearing quite sound.

Thus it appears that voltaic electricity is capable of performing the functions of the nervous power both in the stomach and lungs, and even of preserving the degree of those functions which takes place after death when the nerves have been divided. By depriving these organs of a great part of their nervous power, the reader has seen, the formation of gastric juice is destroyed, and not only the secretions of the lungs disordered, but their structure in the space of a few hours in many parts wholly destroyed. By means of the electric power all these consequences are prevented. The gastric juice continues to be formed, the secretions of the lungs remain healthy, and their structure unimpaired. It remains to be inquired whether voltaic electricity is capable of raising the temperature of the blood. To ascertain this point, the following experiments were made:

Exp. 85. A cup was placed in water of the temperature of 98° of Fahrenheit's thermometer, which was ascertained to be the temperature of the rabbit on whose blood the experiment was made, by placing the bulb of the thermometer in the rabbit's mouth, and allowing it to remain there for two minutes. The temperature of all the rabbits used in the following experiments was ascertained in the same way. Blood was received into the cup from one of the carotid arteries. The bulb of a small thermometer, raised to 98°, and the wires from the different ends of the galvanic trough above mentioned, the whole through being charged, were immersed into it. The blood had been in the cup about two minutes before the whole apparatus was arranged. The same quantity of blood, taken from the same vessel of another rabbit of the same age and temperature, was received into another cup, placed also in water of the temperature of 98°. So far, however, from there being any increase of the temperature from the effects of the voltaic electricity, the blood in the galvanised cup seemed to cool rather faster than that in the

other. The appearance of the blood in the two cups, however, was very different; that in which the wires were immersed assumed a dark venous colour, and most of the coagulum, which had formed more rapidly in this than the other blood, was soon dissolved, the blood again becoming liquid. The blood in the other cup retained the florid colour, and coagulated as usual.

It occurred to me, that the electric power in this experiment had, perhaps, been applied too late to produce all its effect on the blood. For we must suppose the changes of this fluid to commence as soon as it leaves the vessels. With the assistance of Dr. Hastings, and another gentleman, therefore, the experiment wus repeated in the following manner:—

Exp. 86. The rabbits were chosen of the same size and temperature, the thermometer in the mouth of each standing at 98°. The cups were disposed of as in the last experiment; the water, in which they stood, being at the temperature of 98°. Into the one cup nothing was put but the thermometer; into the other, the thermometer and the two wires firom the different ends of the voltaic trough; the thermometer, raised to 98°, being put into the cups at the moment the blood began to flow. Assistants held the rabbits while Dr. Hastings divided the large arteries of the neck. I observed the thermometer, and a person, having a watch marking seconds, noted down the changes of the thermometer as I mentioned them, and the times at which they took place. The experiment was made, first on the blood of the one rabbit, and then on that of the other; but, to save repetition, it is related as if it had been made on both at the same time. The temperature of the mouth is always the same as the temperature of the blood on its first flowing from the vessel. In the cup, in which there was only the thermometer, one minute after the blood began to flow into it, the thermometer stood at 97°, in a quarter of a minute more it stood at 96°, and so on gradually falling; for it is to be observed, that although the cups stood in water of 98°, the air in them was more than ten degrees lower.

In the cup in which the wires were placed, one on each side of the bulb of the thermometer, one minute after the blood had begun to flow into it, the thermometer stood at 100°, in half a minute more at 102°, in half a minute more at 100°, in a minute more at 99°, in half a minute more at 98°; that is, in three minutes after the blood had begun to flow into the cup. After this, the thermometer gradually fell.

While the above changes of temperature went on, the blood in the galvanised cup began to assume a dark colour about the positive wire. But it appears from the preceding experiment that the increase of temperature was not connected with this change of colour, which took place as quickly where no such effect attended it. Besides, the temperature ceased to rise soon after the dark colour appeared about this wire, and the supply of electric power being continued, the dark colour extended till the whole blood in the cup assumed this colour. Air-bubbles arose around both wires. Around the negative wire they continued to rise in such quantity as to form a considerable accumulation of froth. All these appearances took place equally, whether the wires were immersed in the blood at the moment it flowed from the vessel, or after the time had elapsed, at which they ceased to occasion a rise of temperature.

Exp. 87. That I might be assured that there had been no deception in the first experiment, blood was allowed to flow from the large artery of the neck of a rabbit into a cup placed as above, and after it had remained in the cup only about a minute and a half, during which no change of appearance took place in it, the wires and thermometer were immersed into it. The change of colour, and other phenomena mentioned above, took place exactly as before; but there was no rise of temperature; the blood continued gradually to cool.

Exp. 88. In a rabbit, whose temperature was only 96°, both of the large arteries in the neck were divided, and the blood allowed to flow into the cup; a thermometer, raised to 96°, being at the same moment placed in the cup between the galvanic wires. In

a quarter of a minute after the blood began to flow, the thermometer rose to 98°, in half a minute afterwards to 99°. In a quarter of a minute more it had fallen to 98°, in a quarter of a minute it was still 98°, in half a minute more 97°, in a quarter of a minute more, that is, two minutes after the blood began to flow, it returned to 96°; after this it continued gradually to fall. The low temperature of this animal, and the increase of temperature being less than in Experiment 86, probably arose from the same cause.

I wished to ascertain whether voltaic electricity causes any rise of temperature in venous blood.

Exp. 89. Blood was taken from the arm of a person, whose temperature, as appeared by putting the bulb of a thermometer into the mouth, was 98°. The blood was received into a cup placed in water of the same temperature, into which were put the wires from the galvanic trough. The thermometer, raised to 98°, was put into the cup between the wires, as soon as the blood began to flow into it. It continued gradually to sink, at no moment giving the least indication of any rise of temperature. This experiment was repeated in the same way, and with the same result.

Exp. 90. In order to ascertain the effect of voltaic electricity, on blood returning from the viscera, the following experiment was made on a cat, whose temperature was 97°. The vena cava was opened, and the blood from it allowed to flow into a cup placed in water rather above 100°; the thermometer raised to the same temperature, and the galvanic wires being placed in the cup while the blood was flowing into it. The thermometer indicated no rise of temperature. The electric power produced the same visible effects on the venous as it had done on the arterial blood, except that the colour of the former remained unchanged.

In the seventh volume of the Medico-Chirurgical Transactions, Mr. Henry Earle notices cases of palsy, in which the temperature of the paralytic limb, although the pulse was good, was

lower than that of the rest of the body. In the first case which, he mentions, he subjected the limb to the influence of electricity which raised its temperature.

Exp. 91. By the foregoing experiments the idea is suggested that some gaseous fluid probably escapes from arterial blood, soon after it leaves the vessel. To ascertain whether this is the case, a glass of such a shape that the smallest globule of air could be seen in it, was filled with and inverted over mercury. A considerable part of the femoral artery of a large rabbit, whose sensibility had been nearly destroyed by opium, was then exposed and divided under the glass. The blood immediately rose into the glass, and was allowed to remain undisturbed for a quarter of an hour, but no gaseous fluid was disengaged from it. In performing this experiment, if great care be not taken, the hair of the animal and hands of the assistants may convey a little air under the glass, by which we were repeatedly foiled in making the experiment. The artery must not be held deep in the mercury, else the weight of the metal, by compressing it, will prevent the escape of the blood.

The glass, into which the blood was received, rose only about an inch and a half above the surface in the basin. Had it risen sufficiently high to take off any considerable part of the pressure of the atmosphere, the experiment, it is evident, would not have been a fair one. What elastic fluids may be disengaged from arterial blood, when that pressure is removed from it, is a different question; it appears from this experiment, that the difference of the effect of the electric power on the blood at the moment it leaves the vessel, and two minutes after it has left it, does not arise from the escape of any gaseous fluid.

It thus appears that voltaic electricity, as it has been found capable of performing the other functions of the nervous power, is capable also of raising the temperature of the blood.

If the facts I have stated be correct, we can have little doubt that the nervous influence, however modified by the vital powers with which it co-operates, is of the same general nature with the

inanimate agent which was substituted for it; for, to say nothing of the circumstance of the nervous influence being capable of existing in a texture different from that to which it belongs in the living animal, we cannot suppose that there are two distinct powers, the one of which is capable of all the effects of the other; or I would rather say, that such a supposition amounts to a contradiction in terms, because, as it is acknowledged that we know nothing of any principle of action but by its properties, it necessarily follows that by these alone it can be distinguished.

In discussing the nature of the nervous influence, too much has been ascribed to electric tests, which are referred to as if they possessed a power equal to that of chemical tests. A correct chemical test will give evidence of what we are in quest under all circumstances, and is therefore capable in all instances of detecting its presence, and consequently its absence also. This arises from there being but one counteracting power, that of affinity. If the affinity be stronger in the test than in any other substance, the effect of all other affinities is destroyed. We possess no such electric test, because here there may be other counteracting causes beside the power of affinity—opposing currents, for example. Besides, we know that the properties of electricity are so modified by the powers of life, as greatly to interfere with its relations to our tests. The electricity of the torpedo and other electric animals has not as yet affected the common electrometer, yet no one has doubted its identity with the electricity of inanimate nature.

Although electric tests therefore give evidence of the presence of electricity, we cannot by their means prove its absence; a fact with which we should not have been acquainted, were it not, under certain circumstances, possible to prove the presence of electricity without their aid; that is, the presence of electricity may under certain circumstances be proved, where it is not indicated by any of the properties generally admitted to be peculiar to it.

Suppose it were said, for example, that we cannot admit that

electricity is the agent in the combination of oxygen and carbon, because there is no test by which its presence can be detected; the reply of Dr. Faraday, I conceive, would be.—We cannot at present, whatever we may do hereafter, make the electricity employed in effecting this combination evident to any of our tests; but I consider its presence as a necessary inference, because I have adduced facts which prove that, electricity being the agent in all such combinations, if it were not so here, nature would deviate from the simplicity observed in all her other works. Either electricity is the agent in the combination in question, or there are two kinds of chemical affinity.

Under such circumstances, can any other reply avail, except either disproving the facts, or pointing out the fallacy of the inference?

What I have done is strikingly illustrated by the late investigations of Dr. Faraday. It is about five-and-twenty years since I found that voltaic electricity is capable of all the functions of the nervous influence; it now appears from the facts, on which he has founded his doctrine of electro-chemical equivalents, that electricity is the agent in all chemical processes. According to the inferences of Dr. Faraday, therefore, the experiments which prove that the nervous influence is the agent in the functions we have been considering, all of which, we have seen, are chemical processes, are sufficient to prove its electric nature; and we are now also, on the other hand, furnished with direct proof that the brain is capable of collecting and applying, even according to the dictates of the will, the electric power.

Mr. Hunter,[1] whose penetration and originality are ever conspicuous in the midst of his greatest errors, has shown that the blood newly drawn from the vessels of a living animal possesses some of the properties peculiar to living matter, and therefore justly regards it as living, that is, living in the same sense in which an egg must be considered as alive, whilst it can resist the

[1] Treatise on the Animal Economy.

natural changes to which inanimate matter is subject, and by means of an increase of temperature alone produce a living animal. The same degree of cold, for example, will neither freeze an egg newly laid, nor blood newly drawn, which will freeze them after they have been exposed to such causes as tend to destroy life. However frequently an inanimate fluid is frozen and thawed, the same degree of cold is required to freeze it again; but the degree of cold required to freeze blood newly drawn from a living animal is greater than that required to freeze it a second time.[1] It appears from the preceding experiments that the electric power fails to occasion any increase of temperature in blood in about a minute and a half after it has been drawn from the vessel, although no change which we can detect has taken place in it; yet, at the moment it flows from the vessel, this power occasions such an increase of temperature as raises the thermometer three or four degrees, provided the blood has not undergone the effects of the nervous power in the course of circulation. From venous blood the reader has seen it occasions no disengagement of caloric. In the venous state, the blood is returning to have those properties renewed which have been exhausted by the operation of the nervous power. Here also we observe the correspondence of the effects of that power and voltaic electricity, while the blood retains all its vital powers.

The experiments which have been laid before the reader in the present chapter, prove two positions; that the nervous power is not a vital power properly so called, its existence not being essentially connected with the organisation of the part in which it resides; and that voltaic electricity is capable of all its functions.

We have now arrived at the facts which reconcile all the apparent contradictions stated in the first part of this Inquiry.

The heart continues to act for some time after it is removed

[1] See the Experiments of Mr. Hunter in the work just referred to.

from the body, and performs its functions in the fœtal state when neither the brain nor spinal marrow has existed, because it has no dependence on the nervous system, and is only immediately influenced by the removal of the brain and spinal marrow in the perfect animal, in consequence of the failure of respiration.[1]

The heart is supplied with nerves, and subject to the influence of the passions, because, although independent of the nervous system, it is, while the connexion of nerves is entire, immediately subjected to its influence.

Thus, when we remove the brain and spinal marrow, the action of the heart is unimpaired, because it is independent of these organs. When we crush them it is enfeebled or destroyed, because it is influenced through them; and the greater the portion destroyed, and the more sudden its destruction, the greater injury the heart sustains. These facts reconcile the apparent contradictions in the experiments of M. le Gallois.

The heart is independent of the will, because it is exposed to the constantly renewed action of a stimulant over which the will has no control; and because there is no act of volition which could be performed through the medium of the heart.

The functions of the stomach and lungs are destroyed by withdrawing the influence of the brain or spinal marrow, while that of the heart is unimpaired; because the function of the heart depends wholly on the muscular power, which has been found in every part of the body independent of the nervous system; while the function of the stomach chiefly depends on the secreting power, which has been found everywhere dependent on this system. As far as the function of the stomach is muscular, it also continues after the nervous power is withdrawn: all the food

[1] It is evident from all that has been laid before the reader, that could we perfectly imitate the function of respiration after the removal of the brain and spinal marrow, the heart would soon begin to feel the effects of the general failure of the secreting power. The failure of the secreting power in the lungs, indeed, as has already been observed, constitutes the most important difference between natural and artificial respiration.

which is so changed as to become the proper stimulant to the muscular fibres of the stomach is still propelled into the intestine. Thus the excitement of the muscular action of the stomach is indirectly dependent on the nervous system.

The difficulties stated by M. le Gallois respecting the function of respiration disappear when it is admitted, that although the muscular and nervous powers concerned in this function are, as M. le Gallois states them to be, independent of the brain, the sensorial power is here necessary to call them into action: and that the lungs, being supplied with nerves from the eighth pair, the sensorial power must, as far as regards them, cease, when that part of the brain from which these nerves originate, and to which all impressions communicated through the spinal marrow must also be sent, is destroyed. The powers of respiration remain after the destruction of this part, but the sensation which excites the animal to call them into action is gone. With respect to the difficulties which existed previously to the facts ascertained by M. le Gallois, they chiefly relate to the function of the muscular fibre. Haller, we have seen, maintained that its power depends on its own mechanism, but found himself so incapable of replying to his opponents as to be forced into inconsistent admissions. We have seen Haller's opinion respecting the muscular contractility maintained by experiments, which prove that the nervous influence, so far from affording excitability to the muscular fibre, exhausts it, as all other stimulants do; and his difficulties removed by those experiments which point out why, independently of any question respecting the nature of muscular excitability, it is necessary, for the most important reasons, that the muscles of involuntary as well as those of voluntary motion, should be, although in a different way, directly under the influence of the nervous system, and consequently supplied with nerves.

Having now with much labour, and a course of experiments which commenced not less than thirty years ago, cleared away the various impediments which lay in our way, we are prepared to trace the nature of those general states of the system in

which all its functions are more or less concerned, and consequently all its laws more or less called into operation. The states I refer to are those of sleep and death.

Having considered the nature of these states, I shall point out the means by which the system of the more perfect animal is formed into a whole, in consequence of which the affection of any one part tends more or less to affect every other. With this, which will complete the account of the powers of the more perfect animal, the physiological part of the present volume will conclude.

CHAPTER XX.

On the Nature of Sleep.

There is no question relating to the living animal which involves a more general view of its phenomena than the nature of sleep, and, probably, for this reason, none respecting which opinions are more vague and unsatisfactory. I propose to review these various phenomena for the purpose of ascertaining the organs in which its immediate cause exists, the laws on which it depends, and the effects it has on those parts of the system which are not concerned in its production.

We can perceive no final cause of the alternation of watchfulness and sleep, but such as has its origin in the imperfection of our nature. The end of life is enjoyment, and as sleep, if we may not regard it as a positive evil, prevents uniformity in the accomplishment of this end; to say nothing of the occasional inconveniences which attend it, were we as well acquainted with the principles of the animal as we are with those of the solar system, we should probably find this defect, in the nature of things, as unavoidable as the recurrence of darkness and a degree of cold which benumbs, and of heat which overpowers our faculties.

We shall never, perhaps, be able to tell why certain organs are capable of constantly maintaining their functions, while others require intervals of repose; but it is not difficult to perceive the necessity of the former part of the arrangement, because the permanent functions are those on which the life of the animal immediately depends, the intervals of repose belonging to those alone which are the means of intercourse with the world that surrounds him, and which, therefore, have no direct tendency to destroy life.

In tracing the relation of the nervous and muscular systems, I had occasion to point out the different relations which the muscles of voluntary and involuntary motion bear to the nervous system, and that the two sets of nerves, which form the medium of connexion between the active parts of that system, and these classes of muscles, obey different laws; each nerve of the one set conveying the influence of only certain parts of the brain and spinal marrow, each of the other conveying and combining that of every part of these organs. The former, it is now to be observed, while they are associated, on the one hand, with the organs of sense and the muscles of voluntary motion, are associated, on the other, with those parts of the brain and spinal marrow on which the mental functions depend:[1] the latter being associated, on the one hand, with all parts of the brain and spinal marrow, and on the other, with the muscles of involuntary motion and the organs on which life depends.

Thus we find in the more perfect animals two systems in a great degree distinct from each other: the former may be termed the sensitive system, that by which they perceive and act, and consequently are connected with the external world; the latter the vital system, that by which their existence is maintained. To understand the nature of sleep, we must determine the laws peculiar

[1] The reader has seen that the spinal marrow partakes of the sensorial functions. This is very little the case in man, but to a great degree in some animals.

to each of those systems which have relation to that state, and the manner in which each is capable of influencing the other.

When the reasoning powers are fatigued by continued attention, the feelings by the excitement of the passions, the eye by the exercise of sight, the ear by that of hearing, the muscles of voluntary motion by powerful and repeated contractions, &c., the organs of all these functions cease to be excited. In order again to excite them, either stronger stimulants must be employed, or they must be refreshed by repose, during which, the functions of life still continuing, their due degree of excitability is restored; and they thus again become sensible to the usual stimulants of life.

The operation of this law in the sensitive system may be observed under all degrees of excitement. We can perceive a very sensible effect from slighter degrees of exhaustion than that which produces sleep. After sleep there is a vigour which gradually declines till we sleep again; so that every degree of excitement is followed by its corresponding degree of exhaustion. This law of our frame is so prevalent, that physiologists have generally regarded it as belonging to every part of the system. But any degree of excitement which produces weariness, must, by a certain continuance of it, produce inability. It is evident, therefore, that were the organs of life to obey this law, a total failure of their functions must soon ensue. The sensitive system is restored, because its stimulants are withdrawn and the powers of life remain; but if these powers suffer a similar exhaustion, by what means can their restoration be effected, or life itself maintained? This consideration alone might have convinced physiologists that their excitement is regulated by other laws.

It is evident indeed that the circulation continues uninterruptedly; but this has been explained by supposing that the heart and vessels during the intervals of their contractions recover their excitability, the exhaustion of which, during each contraction, has been regarded as the cause of the relaxation which succeeds it.

This theory appeared to apply well to the heart, because during the intervals of its contractions the stimulus which excites it is withdrawn; but we find it wholly inapplicable to the vessels from which the stimulant is never withdrawn, and which can support the motion of the blood, as we seen ascertained by many experiments, independently of the heart. An organ exhausted by the action of any stimulant will never recover its excitability under the operation of the same agent which has exhausted it. The retina will never recover under the same degree of light which has impaired its power, nor the nerve of the ear under the same degree of sound.

Exp. 92. A very simple experiment, however, demonstrates that the theory is as erroneous with respect to the heart as the vessels. If in a newly-dead animal a ligature be thrown around the arteries attached to the heart so that it continues gorged with blood, its contractions, although ineffectual, still continue to recur with the same regularity as before the ligature was applied. When salt is sprinkled on the muscles of the newly-dead animal, the effect is not permanent contraction succeeded by permanent relaxation, but a constant succession of contractions and relaxations, notwithstanding the continued application of the same stimulant, till their power is exhausted.

An experiment, suggested by Dr. Wollaston, and with which he used to amuse his friends, strikingly illustrates the interrupted nature of muscular contraction, even where it is as nearly permanent as the nature of the muscle in its healthy state admits of.[1] If the elbows be made to rest on a table, and the end of a finger of each hand be pressed steadily on that part of the ear which covers the external passage, so as to press it down forcibly on the end of that passage, we hear a rapid succession of distinct concussions. This he ascribed to our thus being made sensible of the motion of the blood in the vessels. But did it proceed

[1] We have reason to believe that in spasm the muscle is in a state of permanent contraction, probably the cause of the pain which attends this state.

from this cause, the repetition of the concussions would correspond with the beats of the heart. That it arises from the rapid succession of the contractions of the muscles of the arm by the action of which the end of the finger is pressed against the ear, may be proved by making the experiment in the following manner. Let the arms rest on the table in such a way as to press by their weight on the fingers which stop the ears, care being taken that the stopping of the ears be left to the weight of the arms, and in no degree produced by the action of the muscles. When we succeed in this attempt, all sense of concussion immediately ceases. It will be found that just in proportion as we succeed in preventing the action of the muscles, the noise abates, and, when we perfectly succeed, ceases altogether. The same property of the muscle may be made perceptible to another of our senses. If a bird be allowed to rest on the finger, we perceive by the finger its weight alone. It so balances itself, that the continued action of its muscles becomes unnecessary. But if the finger be moved, so that the bird is obliged to cling to it to maintain its place, we feel a thrill which consists of the same rapid succession of concussions as in the former instance is perceived by the sense of hearing. The larger the bird is, they are of course the more distinct.

It is quite evident from all that has been said, that the state of the muscle is wholly different in the relaxation which intervenes between the contractions, from that which has supervened when the same stimulus can no longer excite it. Now it is not the first but the last of these states which indicates any loss of power in the muscle.

The whole phenomena of the animal body demonstrate that although it is true that a muscle may be exhausted by powerful and repeated contractions, it is not subject to the law which prevails in the sensitive system, that all degrees of excitement are followed by proportional exhaustion.

Thus it is that the muscles of voluntary motion often suffer exhaustion, because, being under dominion of the will, they are

frequently exposed to excitement which is excessive either in its degree or duration, or both. Their exhaustion does not interfere with health, and for their restoration means are provided in the usual functions of the system. But the muscles employed in the vital functions obey a better regulated stimulant, which never, except in disease, produces any degree of excitement that impairs their power. In many diseases, we see the effect of such excitement. If it does not abate soon, and we cannot by artificial means in a short time reduce it, death is always the consequence: and even a short continuance of it produces a degree of debility that so impairs the powers of life as to render their restoration both slow and difficult. Thus it is evident that on the capability of the muscular fibre to be moderately excited, without suffering any corresponding exhaustion, life immediately depends.

This property belongs equally to the muscles of voluntary as those of involuntary motion, the exemption of the latter from exhaustion in the healthy state of the system, not arising from any peculiarity in the nature of these muscles, but from the circumstances in which they are placed. In many diseases we find the muscles of voluntary motion in a state of excitement, that is, in a state of constant contraction and relaxation, which constitutes their state of excitement, during all our waking hours, that is, during all the time that those parts of the nervous system with which they are associated are capable of exciting them, without a sense of weariness or any other sign of exhaustion in them. The muscles of respiration, which, we have seen, are, in the strictest sense, muscles of voluntary motion, are in a state of constantly renewed and gentle excitement during life. It is only in asthma and other cases, where their excessive action is required, that they experience any degree of exhaustion.

Thus the muscular fibre in its laws of excitement differs essentially from the other organs with which it is associated in the sensitive system. It is neither like them in the healthy state capable of uniform excitement, nor in it are all degrees of excitement followed by proportional exhaustion. But in the vital

system, although all its other parts are capable of uniform excitement, the muscular fibre is not the only organ in which certain degrees of excitement are unattended by exhaustion. The same is true of the ganglionic nerves and those organs of the brain and spinal marrow from which they derive their power, and which, it appears from direct experiment, are distributed throughout the whole extent of both.

The secreting organs indeed, as well as those of circulation, are less vigorous in sleep than in our waking hours; but this, we shall find, besides that a diminished excitement cannot restore impaired excitability, but must, in proportion to its degree, still add to the exhaustion, is the necessary consequence of causes very different from their partaking of the exhaustion which prevails in the sensitive system. It is in disease alone that they suffer any degree of exhaustion, which in them produces a different species of debility, not an exhaustion analogous to that of the sensitive system; which it is even a means of preventing by impeding the functions of life, and thus indirectly proving a cause of irritation to this system, in consequence of which, in many diseases, sleep forsakes us.

It appears from all that has been said, that in the sensitive system alone we find organs subject to exhaustion from all degrees of excitement, and the exhaustion of which is consistent with a state of health, namely, the nerves of this system and those parts of the brain and spinal marrow with which they are associated; but it is a necessary inference from the facts which have been laid before the reader, that the former of these only obey the latter. To the latter alone therefore we must look for the exhaustion which is the immediate cause of sleep.

The parts of the brain and spinal marrow which are associated with the nerves and muscles of the sensitive system, gradually, from the effect of the usual stimulants of life, suffer such a degree of exhaustion that those stimulants can no longer excite them; and their functions, unless stronger stimulants be applied, necessarily cease. Impressions from external objects consequently are

no longer perceived, and therefore cannot produce their usual effects either on mind or body. Thus the expenditure of excitability in those parts of the brain and spinal marrow being arrested, the vital functions still continuing, such an accumulation of it takes place in them as again renders them sensible to the usual stimulants of life; and the activity of the sensitive system is restored.

On the parts of the brain, and, in some animals, of the spinal marrow, as I have already had occasion to observe, which are associated with the nerves and muscles of the sensitive system, the mental functions depend. Hence the phenomena of dreaming, on which I shall make a few observations immediately connected with the other parts of this paper, after considering the manner in which the vital is influenced by the state of the sensitive system in sleep.

We are now to consider the effects of sleep on those organs which have no share in its production.

One of the most important circumstances relating to the state of the sensitive system in sleep is, that its exhaustion is never so complete as, under all circumstances, to prevent its excitement. On this alone it depends, we shall find, that sleep has no fatal tendency. The degree of sensibility which remains in sleep is the distinguishing mark between it and the torpor of disease. That sleep alone is healthy from which we are easily roused. If our fatigue has been such as to render it more profound, it partakes of disease, that is, as will appear more clearly from what I shall have occasion to say of the different species of apoplexy, the vital system partakes of the debility, or some cause is operating which prevents the restoration of the sensitive system.

Distinct as the vital and sensitive systems are, we know that neither can long survive the other. In a paper which appeared in the Philosophical Transactions for 1829, I stated or referred to the facts which prove that in all modes of death, except the most sudden, arising from causes which so impress the nervous

system as almost instantly to destroy all the functions, those of the sensitive cease several hours before those of the vital system, as the reader has seen demonstrated by several experiments which have been laid before him. The animal only dies when his means of enjoyment and intercourse with the world which surrounds him, no longer exist. This consequence is constant and never long delayed. It is necessary, therefore, to a clear view of the state of the functions of the animal body in sleep, to determine the bonds of union between the sensitive and vital systems, at first view so distinct, which render their existence, except for a limited time, inseparable.

That the sensitive cannot exist independently of the vital system, is evident on the slightest consideration; but the dependence of the latter on the former is much less so. The facts which have been laid before the reader prove that in the more perfect animals, the function of respiration, being the only vital function which requires the co-operation of the sensitive system, is here the bond of union. It appears from those facts that the muscles of respiration are, in the strictest sense, muscles of voluntary motion, the excitement of which consequently depends on the powers of that system. When the power of sensation wholly ceases, we cease to breathe.

So confused have been the ideas of physiologists on this part of the subject, that to account for the continued action of the muscles of respiration, and their intimate connexion with the vital system, they have supposed a third class of muscles partaking of the nature of both the others, those of voluntary and involuntary motion, to which it has been alleged the muscles of respiration belong. If this be the case, these muscles must change their nature every instant, because they are the same muscles which are employed in a thousand other acts universally acknowledged to be the mere acts of volition; and, on the other hand, when powerful causes impede the breathing, all the muscles of the trunk are employed in this function. Besides, the facts which have been laid before the reader prove not only that there is

no such class of muscles as that here supposed, but that the laws of excitability are the same in all muscles, the difference between the muscles of voluntary and involuntary motion depending wholly on the nature of their functions and the circumstances in which they are placed. The nervous influence, although equally capacle of influencing both, is supplied to them in different ways and for different purposes, the usual functions of the muscles of voluntary wholly, of involuntary motion in no degree immediately depending on the sensitive system. The action of the muscles of respiration continues during sleep, because the exhaustion of the sensitive system is not complete, and the cause which influences this system in their excitement, continues in our sleeping as well as waking hours; and the same is true of all other muscles of voluntary motion, as far as the causes which induce us to excite them are applied. In the soundest sleep we move our limbs, if their posture be rendered uneasy. Are we not obliged by the easy nature of our beds to guard against such causes as might produce this effect in sleep, unless our exhaustion is such as to partake of disease, else the motions they would excite would quickly rouse us? Those of respiration are too gentle to produce this effect.

The only change which takes place in the action of the muscles of respiration during sleep is, that in proportion as the sensibility is impaired they are necessarily excited less readily, and the act of respiration is thus rendered less frequent, a more powerful application of the cause being required; the consequence of which is, that when they are excited, the air is drawn in with greater force; hence, and from the relaxation which is apt to take place during sleep in the parts about the fauces, particularly in those advanced in life and those of relaxed habits, the cause of snoring. Thus we generally observe that the snoring is the louder the slower the breathing; that is, the relaxation of the fauces being the same, the more profound the sleep. The loudest snoring I ever heard, so loud as to startle the attendants, was in the last ten minutes of the life of a person who died of a disease

of the brain impairing the sensibility, and who only breathed three or four times during that space.

The other changes observed in the vital system in sleep are evidently the consequence of the diminished frequency of respiration. This necessarily produces a proportionate diminution in the frequency of the pulse; the properties of the blood being less frequently renovated in the lungs, it less readily excites the heart and vessels, and the diminished force of circulation is as necessarily attended with a diminished formation of the secreted fluids. The state of the vital organs, in its turn, influences the sensitive system, and thus the sleep is rendered more perfect. While health continues, however, no change takes place in the vital powers to prevent the perfect restoration of those functions by which the animal is again fitted for intercourse with the external world, for, as appears from what has been said, these powers are never impaired in sleep, but only less readily excited, from the temporary diminution of the causes of their excitement.

The foregoing positions are well illustrated by the symptoms of apoplexy, in which a cause exists that prevents the restoration of the sensitive system, and which consequently points out to us in a more striking manner the influence of the sensitive on the vital system. Here we find that in proportion as the sensibility fails, the respiration, and with it the pulse, continue to become slower; and when it has failed altogether, so that no cause of irritation can excite any sensation, the respiration ceases, and the loss of circulation soon follows. In this way the patient dies in the most unmixed cases of sanguineous apoplexy, where the cause of derangement is a gradually increasing pressure on the brain, in consequence of which its sensibility is at length extinguished. Here there is no original disease of the vital organs. Could the sensibility be sufficiently maintained to preserve a due frequency of respiration, and nourishment from time to time be introduced into the stomach, life would go on as in sleep, unless the increasing affection of the brain, extending from the sensitive to the vital parts of that organ, so deranged the assimilat-

ing processes as to destroy life in this way. In all ordinary cases of sanguineous apoplexy, this more or less takes place. The diseased state of the sensitive, in consequence of the mutual sympathy of all neighbouring parts, speads more or less to the vital part of the brain and spinal marrow; for it is rare for the cause, as in the case above referred to, to continue to the last wholly confined to the sensitive system, so that the immediate cause of death is merely the ceasing of respiration in consequence of a total loss of sensibility.

The accumulation of phlegm in the lungs in apoplexy arises from the assimilating processes being deranged by the failure of that part of the nervous influence supplied by all parts of the brain and spinal marrow to the vital organs, in consequence of the vital parts of the brain partaking of the cause of the disease. I shall have occasion, in the next chapter, to recur to and further illustrate this part of the subject. I have repeatedly, in sanguineous apoplexy, removed the accumulation of phlegm, the breathing becoming as free as in health, by causing voltaic electricity to pass through the lungs in the direction of their nerves. This, it is evident, can have no direct tendency to remove the disease, although by its means life may often be prolonged, and thus more time afforded for the application of the means of cure; this accumulation of phlegm greatly impeding the due change of the blood in the lungs, and thus conspiring with the diminished frequency of respiration to deprive it of its vital properties.

A short comparison of apoplexy from compression, with that which is with great propriety termed nervous, will throw additional light on this part of the subject.

It is shown by experiments detailed in papers which appeared in the Philosophical Transactions for 1815, and the results of which have been laid before the reader, that although the power of the heart and vessels is independent of the brain and spinal marrow, causes operating on these organs are capable of influencing them, and that even to the total destruction of their power. When, therefore, the cause of apoplexy, instead of being

a gradually increasing pressure of the brain,—which, we have seen, was found by experiment, however powerful it may be, to have no direct influence on the action of the heart,—is of such a nature as, while it impairs the sensibility, also directly impairs the power of the heart and blood-vessels, we have a disease of a very different nature from apoplexy from mere compression. In the latter, if we can remove the cause of pressure and prevent its recurrence, we invariably cure the disease. There is no other cause of derangement. The vital functions are only impeded by the want of the due change of the blood in the lungs, in consequence of a less frequent respiration; and as far as the cause of the disease spreads by sympathy[1] to the vital parts of the brain, causing a deranged state of the assimilating functions.

Death in sanguineous apoplexy is necessarily slow, because it always requires some time for the gradually increasing pressure either to destroy the sensibility, and consequently wholly stop respiration, or, through the vital organs of the brain, so derange the assimilating processes as in this way to prove fatal; for, from some peculiarity in the cause, the effect of which more readily than usual spreads to the vital parts of the brain, death, in apoplexy from compression, sometimes appears rather to arise from this derangement than the loss of sensibility, the phlegm gradually accumulating in the lungs till it wholly prevents the necessary change of the blood effected in them.[2]

But when the cause which impairs the sensibility, also through the ganglionic system immediately enfeebles the heart and blood-vessels, the course of the disease is very different. We have here a cause at once impairing the powers of circulation; and when it is excessive, death is often instantaneous. Such is the cause of

[1] In the following, the practical part of the present volume, it will be necessary to enter at length into the laws of sympathy, on which the phenomena of disease so extensively depend.

[2] This accumulation of phlegm in the lungs, we have seen, has been found experimentally to be the uniform consequence of lessening the supply of their nervous influence.

death from blows on the head, which, when not sufficient to produce instant death, produce what is called concussion of the brain, in which, from the immediate influence of the diseased state of the brain on the heart and blood-vessels, a state analogous to syncope is combined with impaired sensibility. The circulation is doubly assailed by the direct diminution of the power of its organs, and a failure in the stimulating power of the blood, in consequence of its less perfect renovation in the lungs; and the former, being often the more powerful cause, obscures the effects on the vital organs of the latter. The pulse, instead of being slow but regular, and of unimpaired strength, is feeble, irregular, and fluttering, and a general paleness of the surface indicates a degree of failure of circulation, far beyond what is observed in cases of compression.

All sudden and excessive affections of the brain may produce the same effects as a blow on the head. Thus people have instantly expired from fear, rage, or excessive joy; and thus in the mobs of Lord George Gordon, some, from the sudden effect on the brain through the nerves of the stomach, expired on taking a draught of spirit of wine, which they had mistaken for common gin.

But it is not necessary that the cause, as in these cases, should be either sudden or violent to produce this species of apoplexy. A long-continued recurrence of slighter causes weakening the powers of the brain, often, along with them, gradually impairs those of the heart and blood-vessels, in the same way that an infusion of tobacco, applied to the brain in the experiments above referred to, impaired their powers. These are the most common causes of nervous apoplexy; and in proportion as their operation has been slow, the course of the disease is less rapid.

Thus we see it supervene in those who have been long exposed to the irritations which attend the more serious and confirmed cases of indigestion or long-continued causes of anxiety, particularly in gouty habits, in which there is often a great tendency to debility in the vital organs; and we readily perceive,

from what has been said, why apoplexy from such causes is so generally fatal. The powers both of the nervous and circulating systems are undermined, and with them the secreting and other assimilating processes which depend on them. The powers which ought to respond to our remedies have failed. Our efforts, therefore, are for the most part equally unavailing, in restoring either the sensibility or the powers of circulation, and both are necessary to recovery.[1]

From a review of the whole of the facts which have been laid before the reader, it appears,—

1. That in the brain and spinal marrow alone reside the active parts of the nervous system.

2. That the law of excitement in the parts of these organs, which are associated with the nerves of sensation and voluntary motion, is uniform excitement followed by proportional exhaustion, which, when it takes place to such a degree as to suspend their usual functions, constitutes sleep; all degrees of exhaustion which do not extend beyond them and the parts associated with them, being consistent with health.

3. That the law of excitement in those parts of the brain and spinal marrow which are associated with the vital nerves is also uniform excitement, but which is only, when excessive, followed by any degree of exhaustion, no degree of which is consistent with health.

4. That the vital, in no degree partaking of the exhaustion of the sensitive system in sleep, only appears to do so from the influence of the latter on the function of respiration, the only vital function in which these systems co-operate, in consequence of which its organs, without being in any degree debilitated, are less readily excited.

[1] In the following chapter it will be necessary to enter much more minutely into the nature of nervous apoplexy, which we shall find to constitute the last stage of an extensive class of diseases, and in the last, the practical, part of this volume, some further observations on the same subject will be necessary.

5. That the law of excitement of the muscular fibre, with which both the vital and sensitive parts of the brain and spinal marrow are associated, is interrupted excitement, which, like the excitement of the vital parts of these organs, is only, when excessive, followed by any degree of exhaustion. And,

6. That the nature of the muscular fibre is everywhere the same, the apparent differences in the nature of the muscles of voluntary and involuntary motion depending on the differences of their functions, of their relation to the brain and spinal marrow, and of the circumstances in which they are placed.

I shall close this paper with a few observations on dreaming, immediately connected with the preceding parts of the subject.

Had we, independently of experience, been made acquainted with the nature of sleep, we might have foretold that dreaming—pretty much as we find it—would be its consequence.

We here find the sensitive parts of the brain, to which the powers of mind belong, in a state of exhaustion, but not such exhaustion as prevents their being excited by slight causes, while other parts of the system are still in a state of activity. But it is only in the most perfect state of health, and such as we rarely enjoy, that the vital functions are performed without slight causes of irritation arising in some of their various and complicated processes, which tend to disturb the repose of the sensitive parts of the brain. Thus it is that indigestion and other internal causes of irritation produce dreaming. Such causes are partial, and therefore only partially excite those parts.

It seems greatly to influence the phenomena of dreaming, that in order to favour the occurrence of sleep, and thus, as far as we can prevent unnecessary exhaustion, means are always employed, at its accustomed times, to prevent, as much as possible, the excitement of the external organs of sense, and consequently those parts of the brain associated with them. This renders us the more sensible to causes of excitement existing within our own

bodies, while, by the inactivity of the parts of the brain which are associated with those organs, we are deprived of the usual control over such parts of the mental functions as are thus excited; the effect of which is greatly increased by the rapidity of the operations of the memory and imagination, when not restrained by some of the various means employed for that purpose in our waking hours. These are often objects of the senses, as written language, diagrams, sounds, and sometimes even objects of touch; but the most common is the use of words, independently of any object presented to our senses.

Any one may easily perceive how difficult it is to pursue a train of reasoning without this means of detaining his ideas, for the purpose of steadily considering them and comparing them together. Now, in sleep, in consequence of the excitement of the brain being so partial, we are deprived of all these means: and our ideas pass with such rapidity as precludes all consideration and comparison. Our conceptions, therefore, are uncorrected by experience, and we are not at all surprised at the greatest incongruities. Why should we be surprised at our moving through the air, when we are not aware that we have not always done so? The mind of the dreamer differs from that of the infant in having been variously impressed, and therefore in the capability of having its impressions recalled. But it is only as far as it is excited, that any impression can be recalled. With this exception, it is as void of the results of experience as the mind of the infant, and therefore, in its partial excitement, of the means of correcting any train of ideas which that partial excitement suggests. In general, there is neither time nor subject for reflection, and, consequently, there can neither be doubt nor hesitation.

Such is the rapidity of our thoughts in dreaming, that it is not uncommon for a dream, excited by the noise that awakes us, and which, therefore, must take place in the act of awaking, to occupy, when put into words, more than fifty times the space in the relation. It is a good illustration of what is here said, that when

we dream that we are conversing, and are thus obliged to employ words, the usual incongruities of dreaming do not occur. The ideas are sufficiently detained to enable us to correct the suggestions of the imagination. No man ever dreamt that he was telling another that he had been flying through the air.

Thus the peculiarities of dreaming arise from the partial operation of the causes of disturbance, and some of the sensitive parts of the brain being capable of excitement without disturbing the others; and thus it is, that the more near we are to awaking, the more rational our dreams become, all parts of the brain beginning to partake of the excitement; which has given rise to the adage, that morning dreams are true.

CHAPTER XXI.

VII. *On the Nature of Death.*

I NEED hardly say, that in such a work as the present, I have no intention of entering into the part of the subject of this chapter which may justly be termed metaphysical. The veil which separates it from experimental science must ever remain impenetrable, there being no source of information respecting it, but a direct revelation from the great Author of our being, or the instincts he has implanted in our nature, for all knowledge is not acquired. We come into the world with knowledge essential to our existence. The infant knows as well how to breathe and how to suck as the adult, and these acts depend as much on mental operations as those which are the results of experience. He perceives his wants, and he knows how to relieve them; and the extent to which this species of knowledge exists in some animals, whose reasoning powers are extremely limited, justly excites our wonder and admiration. They know what is essential to their condition, with an accuracy which sets at defiance all

the efforts of human reasoning; for their knowledge is the knowledge of their Creator.

To the physiological part of the subject alone I wish to direct the attention of the reader. It forms part of the same subject with the preceding parts of this volume; namely, the nature of the various powers of the living animal body, and the relations which, under different circumstances, they bear to each other. I have endeavoured to trace the effects and the nature of their influence on each other while their state of vigour remains; in the following chapter I shall attempt to point out the manner in which they influence each other in their state of decay, and the effects which thence arise.

In the course of my Inquiry into the Laws of the Vital Functions, it became necessary, we have seen, to determine, with more precision than had been done, the line of distinction between the sensorial and nervous functions.

The function of the muscular system, from its nature and the peculiar structure of its organs, is readily defined; but in the nervous system we perceive more than one set of functions, and yet, both from the variety of ways in which they are interwoven, and from the peculiar mechanism of the active parts of their organs being so minute as to escape our senses, and consequently the investigations of the anatomist, the difficulty of correctly distinguishing them is considerable. It is only by experiments instituted for the purpose, and founded on the very different nature of these sets of functions, that the line of distinction can be drawn.

In order to render the results more certain, I endeavoured to ascertain this line by two sets of experiments, conducted on different principles; the object of the one being to ascertain what functions remain after the sensorial power is withdrawn, and of the other, what functions fail on withdrawing the nervous power; and in prosecuting this subject, as appears from what has been said, I found it requisite to study the process of dying,

to determine the steps by which the body of the more perfect animal becomes subject to the laws of inanimate matter.

I have hitherto, however, entered no farther into the nature of death than was necessary for the purpose I at the time had in view. I am now about to compare the results of the various experiments which bear on the subject, with a view, as far as experiment can apply to it, of explaining the nature of death.

It appears to me that the various facts ascertained in the course of the inquiries in which I have been so long engaged throw light on this subject. I shall, as I proceed, refer to the passages, either in my papers in the Philosophical Transactions, or the present Inquiry, where the proofs of the different positions I shall have occasion to state, will be found.

In the preceding chapter I had occasion to observe, that there is no question relating to the animal economy which involves a more general view of its phenomena than the nature of sleep. The nature of death also includes a general view of the functions of health, for such we shall find are the laws of our frame, that these functions alone necessarily lead to death; but the nature of death is a more complicated question. It includes the various ways in which the functions are influenced by disease, the effects of which are so numerous, that they seem at first view a train of countless phenomena which defy all attempts to refer them to general principles.

I need not say that many advantages will arise from a correct knowledge of the immediate cause of death, and of the different sources from which the state that constitutes that cause arises. The most important are, that it will give to the physician a clearer view of the tendencies of disease, and consequently of the indications of cure; but it is not the least of its advantages, that it strips a change which all must undergo, of the groundless terrors with which the timid and fanciful have clothed it.

It appears from the experiments in question, that in the more perfect animals there are four distinct classes of functions—the sensorial, the nervous, the muscular, and the functions of the

living blood; which, having no direct dependence, are yet, through their organs, dependent on each other; for the destruction of any one of these classes of functions more or less immediately deranges the structure of the organs of all.

We know that the immediate organs of the nervous and sensorial functions, although both residing in the brain and spinal marrow, are distinct sets of organs, because they have not the same locality; the former, as appears from direct experiments, being distributed throughout the whole brain and spinal marrow, and, as far as experiment can determine, equally so, except that the lower part of the spinal marrow either partakes of them less, or they are there of less power; while the latter, in all the more perfect animals, are chiefly, and in man almost wholly, confined to the brain; and we have seen all the functions of the nervous power remaining after the sensorial functions had finally ceased, and only indirectly impaired by their loss.

The sensorial is the leading power in the sensitive functions, those by which we perceive and act,—and consequently are connected with the world which surrounds us: the nervous, the leading power in the vital functions by which we are maintained; the muscular power and the powers of the living blood, and in each system the leading power of the other, acting subordinate parts: for it appears from the experiments which have been laid before the reader, that all the powers of the living animal are employed in both systems.

What in common language is called death consists in the loss of the sensorial functions alone, the nervous powers and the powers of the living blood still in a greater or less degree remaining, which are gradually lost in consequence of the failure of respiration, the only vital function to which the co-operation of the sensorial power is necessary; that is, provided death be not caused either by sudden and excessive loss of blood, or by an instantaneous destruction of the sensorial or nervous organs; for, under such circumstances, the whole powers of the more perfect animal, we have seen, are at once destroyed.

When the animal no longer feels and wills, his breathing ceases, and he is, according to the common acceptation of the term, dead; although his body still retains its other powers, which, while they last, prevent its obeying the laws of inanimate nature; but the changes which after this take place, of course no more affect the individual than if they took place in any other mass of matter.

In inquiring into the physiological nature of what is called death, therefore, it is to the ceasing of the sensorial functions alone that the attention must be directed. Thus the subject divides itself into two parts: the final loss of the sensorial functions, which in common language has obtained the name of Death; and absolute death, that is, the loss of all the functions, which we shall find in the more perfect animals is the necessary consequence of the loss of the sensorial functions.

The latter functions, we have seen, belong to those parts of the brain and spinal marrow which are associated with the nerves of the sensitive system; and are the parts of that system which maintain the functions of all its other parts. To them, therefore, we must look for the immediate cause of failure when the functions of the sensitive system, whether temporarily or finally, fail. It is here we found that the immediate cause of sleep exists; and it appears from what has just been said, that to the same parts we must look for the immediate cause of what is called death.

The state which immediately precedes the last act of dying, then, according to the common acceptation of the term, and sleep, depend on a failure of function in the same organs. In what, then, consists the difference of these states? The most evident is, that the one is a temporary, the other a final failure; and it will appear that, in the only death which can strictly be called natural, the state of the sensitive system which immediately precedes death differs from its state in sleep in no respect but in degree.

The cause of sleep, as appears from what has been said, is uniformly the same,—a diminished excitability of the sensitive parts of the brain and spinal marrow, in consequence of the action of the ordinary stimulants of life; but a loss of excitability in

those parts, we shall find, is never the sole cause of death, and in certain cases makes no part of its cause. In sleep we have seen that the sensitive parts of the brain and spinal marrow regain their functions in consequence of the continued vigour of the vital system, by which their excitability is restored. To render the exhaustion which constitutes sleep permanent, therefore, the powers of this system also must fail; and if any cause of failure in these powers occur, it is evident that whatever be the state of the sensitive system, its powers must fail with them.

The natural death of the animal is the death of old age; and as this is the simplest form of death, it is that which I shall first consider. We shall find that the state which immediately precedes this death, and must consequently be considered as its cause, must, in the nature of things, differ from sleep in no other respect than the less vigorous state of the functions of both systems, and consequently that these states are identical; the greater or less general vigour makes no difference in their nature.

We are not necessarily born to suffering. All natural states, with the exception of child-bearing, (and in its most natural state even this is hardly an exception,) are more or less pleasurable. It will appear from the nature of our constitutions, that the last feelings in natural death are necessarily of the same nature as those which precede sleep. It is only where the course of our decay is disturbed, that suffering of any kind attends it.

From a knowledge of the animal economy, we might, independently of experience, have foretold that a state of sleep would be that which immediately precedes the last act of dying from old age.

It appears from what was said of the nature of sleep in the last chapter, that although the vital organs do not, in it, partake of the peculiar state which constitutes sleep, their functions are all, for the time, impaired by the exhaustion of the sensitive system. The respiration, we have seen, is rendered less frequent, in consequence of which the activity both of the circulation and the other assimilating functions which depend on it, is, for the time lessened.

Now, as the death of old age arises from the gradual failure of those functions, it must necessarily take place at the time at which their vigour is most impaired. If the vital powers are still capable of restoring the sensitive system under the disadvantage of a diminished frequency of respiration, it is evident that, if their decay be gradual, nothing occurring suddenly to accelerate it, they cannot fail to maintain the functions of that system during the short time which intervenes, before the recurrence of sleep again exposes them to the same difficulty. Their failure necessarily takes place at the time when their functions are most difficult. The death of old age, therefore, is literally the last sleep, uncharacterised by any peculiarity. The general languor of the functions in the last waking interval is attended with no peculiar suffering, and the last sleep commences with the usual grateful feelings of repose, the last feelings experienced; for with what takes place after them, the feelings, being suspended, have no concern.

The only difference between the last and the sleep of former times is, that the exhaustion of the sensitive system, which is at first, as in the latter case, only partial, (for in the beginning of that sleep the sleeper may be roused by more powerful stimulants than those which preceded it,) becomes in its continuance, in consequence of the failure of those powers which formerly restored the sensitive system, complete.

As it is by the continued action of the vital parts in sleep that the sensitive parts are restored, the less active the former become, they necessarily effect their restoration the less readily; and when they can no longer effect it, the individual awakes no more; but the circumstance of the vital being no longer capable of restoring the sensitive system makes no alteration in the nature of its exhaustion. It is still, while it lasts, the same exhaustion which constitutes sleep. The sleep proves final, but the sleeper is wholly unconscious of the cause which renders it so; and there is nothing in its commencement to inform us whether it will be final or not. Thus the sensibility is extinguished, and consequently

respiration ceases. The extinction of the sensibility is the last act of dying, in the common acceptation of the term. As the ordinary stimulants of the day produce the sleep of daily occurrence, those of life produce the sleep of death.

Although the sleep of each day restores the sensitive system from the exhaustion which causes it, the daily recurrence of the exhaustion has the effect of permanently lessening the excitability of that system; a change not to be perceived from day to day, but which, from many phenomena, becomes sensible in the course of years. As the sensitive system becomes less excitable as the day advances than on first awaking, in like manner it becomes less excitable, as life advances, than in childhood; and in like manner, as the repeated excitement of the sensitive system tends to the final decay of its sensibility, the continued excitement of the vital system, as we might *à priori* have supposed, has a similar tendency with respect to the excitability of this system. We find the pulse becoming slower as we advance in life, in consequence of the lessened excitability of the heart and blood-vessels, and the vital organs less readily influenced by the parts of the nervous system associated with them, proving that their functions are also under the process of decay. On these parts and the powers of circulation all the assimilating processes depend; and the shrinking frames of the aged indicate their weakened state, and the approach of their final extinction; for those were deceived who taught that there is nothing in the laws of our frame which should lead us to believe that it is not formed to last for ever.

In all forms of death we shall find that the immediate cause of absolute death, which is very different from what we call death, is a loss of power in the organs of the leading power in the vital system.

The greatest degree of excitability, either in the sensitive or vital system, is not that which produces the most vigorous state of health. We may be too excitable, as well as too little so. Many of the more serious diseases of children arise from this cause. The derangement of the digestive organs, which in the

adult produces the nervous irritations of indigestion, produces in the infant inflammation of, and effusion on, the brain. The irritation of the gums, which produces pain and restlessness in the former, in the latter produces convulsions and death. Thus it is that the habit of the child is less firm and vigorous than that of the adult, which has acquired steadiness by the diminution of its excitability, in consequence of the continued action of the stimulants of life; but, after a certain period, the fault is a deficiency, not a redundance, of excitability, a defect apparently the necessary consequence of the laws of our frame, and to which every day unavoidably adds.

The redundance of excitability in children, the cause of many evils, we may be assured answers some important end. There is reason to believe that it is on it that the growth of the body depends, and that the due proportion between the excitability and the stimulants of life, by the gradual diminution of the former, determines the period at which the growth is completed in each individual. While the excitability continues redundant, the ordinary stimulants of life necessarily support a greater activity of the functions than is required for the mere maintenance of the body, and thus its volume enlarges, on the same principle that we have just seen it shrinks in the aged, in consequence of their excitability having become defective. It seems to be on this principle, namely, by a premature exhaustion of the excitability, that the hardships of life, that is, the greater than usual applications of its stimulants, check the growth. On the same principle we should expect to find that the growth would cease soonest in the most excitable habits, because in them the excitability will soonest be reduced to a due balance with the stimulants of life. Thus it seems to be, that the growth of women, who are more excitable than men, generally stops sooner, and consequently that they are of shorter stature, large women, for the most part, having less of the habit peculiar to their sex; and that by far the greater number of the most excitable men, who, in consequence of this constitution, make the greatest figure in their day, are

men of short stature, while giants are generally of an opposite habit of body. There must, of course, to such rules be many exceptions. Where so many causes are operating, no result can be uniform.

That above described is the only form of death which, strictly speaking, can be regarded as natural. In all its other forms the regular course is disturbed by adventitious causes. But the causes which interfere with the regular course of nature, and which make their impression either directly on our bodies, or through the medium of our mental powers, are, in civilised society, so numerous and complicated, that it is rare to see an instance of such a death as that above described. At whatever period death arrives, it is almost always the effect of disease; and at advanced periods of life we only become more liable to death in consequence of our weakened powers rendering us more subject to disease.

Of the various instances of death I have witnessed, there was none that could be regarded as wholly the effect of age. It was always possible to point out some one or more of the vital organs more deranged than the rest, to which death was chiefly to be ascribed. We have, however, accounts of death from old age alone, which were such as has just been described, so that the inferences afforded by the laws of the animal economy are here confirmed by experience.

If we wish to prolong life, we must keep the attention so far directed to the health as to watch the first tendency to failure in any of the vital functions. In a great majority of instances, to a very late period of life, the failure in the commencement is capable of being corrected. It becomes obstinate, by the power of habit, and by the laws of sympathy complicated; and on both these accounts difficult of cure. We may be assured there is, in all, the capability of long life, if they can escape the effect of disease. Thus it is that those who lead a quiet and retired life, little exposed to powerful impressions either of mind or body, often attain a great age. It is an

additional motive for watching the state of health at advanced periods of life, that the longer we live, the less in general is our suffering at the last; the nature of death partaking the more of that old age. For the further consideration of this subject I beg to refer to my Treatise *On the Preservation of Health, and particularly the Prevention of Organic Diseases.*

All modes of death, with the exception of that from old age, may be regarded as more or less violent; but, in considering their nature, we must not confound the last act of dying with the suffering which precedes it, and which is often no less when it terminates in recovery than in death, which equally relieves it; and as death, in the usual acceptation of the word, from whatever cause it arises, consists in the loss of the sensorial functions alone, the act of dying is, in this respect, in all cases essentially the same. In all my experiments I found the nervous and muscular functions and the powers of the living blood surviving the sensorial functions. Even in cases of instantaneous death, some degree of this succession of events may be observed.

When the animal no longer feels and wills, he is what we call dead; but for a certain time the motion of the blood in every part of the system still continues, and all the assimilating functions still go on, as may be demonstrated, we have seen, by dividing the vital nerves immediately after death, which produces the same change of structure in the organs supplied by them, though in a less degree, as during the life of the animal; and that all this would be the case, a knowledge of the laws of the animal economy would have told us, independently of the aid of experiment, if we could, without this aid, have acquired it.

The removal of the sensorial powers neither destroys the muscular power, nor deprives the muscles of involuntary motion of the stimulus which excites them. The heart, indeed, is incapable of its function, because, from the interruption of respiration, its left side is no longer supplied with the kind of blood which is its natural stimulant; and the accumulation of blood in the lungs

from the same cause affecting a great proportion of the vessels, prevents the right side from emptying itself. These are the necessary and almost immediate effects of the interruption of respiration; but the change in the blood of all the capillaries, with the exception of those which belong to this class of vessels, necessarily takes place more slowly. A certain time must always elapse before the stoppage of respiration greatly affects it. It has been sent to these vessels more or less in its proper state, and it still finds its vessels capable of being influenced by their usual stimulant. Thus, as I have ascertained by many experiments, the motion of the blood continues in these vessels for several hours after respiration has ceased, that is, as long as the blood can be drawn from the larger arteries—the cause, we have seen, of these arteries being found empty some time after death.

But this is not all; the nerves of the ganglionic as well as cerebral system retain their power for a certain time after the supply of that power from the brain and spinal marrow has ceased. The blood, therefore, still finds the secreting surfaces in a state more or less capable of their functions, and the secreting processes, as I ascertained by frequently repeated experiments, still go on; nor is even this all, for the brain and spinal marrow depend for the continuance of their functions on the same powers as other organs; and I found, as the reader has seen, by an experiment made on so large a scale that it was impossible to be deceived in the result, that there is an actual supply of nervous influence after the sensorial functions have ceased—that is, after what is called death.

Such is the natural decay of our frames; but, as I have already had occasion to observe, it is very rare for it to run its course uninterruptedly, particularly in civilised life. It is almost always disturbed by adventitious causes accelerating it; often the decay of particular parts, which, in consequence of the mutual dependence of the various functions, disorders the whole. Although these causes are of infinite variety, the laws of our frame are

limited, and, therefore, many must operate on the same principle. This leads us to believe that, however varied the causes of disease, it may be possible to reduce their more ultimate effects to a few general heads. The exhaustion of the sensitive system, for example, is of the same nature, whatever be the cause of excitement; and other forms of debility, affecting either the sensitive or vital system, cannot be very various, however various the causes which produce them. We have reason to believe that the endless variety of disease depends more on the peculiar nature and functions of the different organs affected, and the peculiar manner in which different causes affect them, than on any great variety in the states which constitute the more immediate causes of death. However various the effects of disease, there must be but a few points to which they all tend, because the last in the chain of causes which produces what is called death, we shall find is always the same, and seated in the same parts. On these principles we may hope to reduce the effects of the adventitious causes of death to a few heads, and thus to obtain such a view of the subject as shall enable us to trace the nature, and consequently the operation, of the causes of our decay in individual instances, and therefore to perceive more clearly the operation of the means which tend to counteract them. In the prosecution of this subject I shall commence with those causes of disease whose operation most resembles that of the wholesome stimulants of life; and in pursuing, by means of the various experiments which tend to unfold the laws of the animal economy, the consequences of these causes, we shall be led to the effects of such as have nothing in common with them.

It appears from what was said of the nature of sleep, that all degrees of excitement in the parts of the brain and spinal marrow, associated with the nerves of the sensitive system, are followed by proportional exhaustion. The only limit to this law is the capability of bearing in those parts. Exhausted by mental

excitement, the criminal is often awakened for his execution; and the soldier, both by mental and bodily excitement, sleeps by the roaring cannon.

Now, although the usual stimulants of the day never, except in old age, where we have seen all our powers have long been in a state of decay, produce such exhaustion as to endanger life; the exhaustion from stimulants of greater power cannot with safety be frequently repeated; because by their continued operation the sensitive parts of the brain and spinal marrow, being both more exhausted than is consistent with the due state of the functions before sleep takes place, and roused before they have been refreshed to the usual degree by repose, a state of disease is induced; and all diseased states affecting the system generally, if their causes continue to operate, necessarily prove fatal.

Although in ordinary sleep the vital functions are for the time impaired, in consequence of the lessened sensibility rendering the act of respiration less frequent, the state both of the vital and sensitive system is as much a state of health as in our waking hours. The insensibility of the latter only extends to the effects of the daily stimulants of life; and there are ample means in the functions of health for the restoration of this system,—the powers of the vital system, as I have already had occasion to observe, being in no degree diminished, but only, in consequence of a slower respiration, less readily excited.

As soon as a diseased state of the sensitive system is established from the causes just mentioned, it begins to affect the vital system otherwise than through the intervention of respiration, the only medium, we have seen, through which the healthy exhaustion of the former affects the latter; for such is the sympathy between the sensitive and vital parts of the brain and spinal marrow, that any deviation from the healthy state of either is immediately felt by the other.

The characteristic of the mode of death I am considering, is the tendency of its causes to produce sleep in the first instance. So far their operation is the same, but greater in degree, with the

common stimulants of life. At this period, if the cause of suffering be removed, the sleep is only more profound than on former occasions; and, as on them, it continues till the sensitive system again becomes obedient to those stimulants; if not, this system soon partakes of a species of debility so different from the healthy exhaustion, that instead of being relieved by the continued action of the vital parts of the brain and spinal marrow, it spreads to them. Hence the nutritive and other vital processes begin to fail, and the various irritations which attend their failure still further contribute to the debility of the sensitive system, and consequently, indirectly, to increase the cause of their failure. The derangement of each system thus aggravating that of the other, the evil proceeds not by simple addition, but in an increasing ratio, till all their powers are extinguished.

Whatever be the suffering which precedes what is called death, the moment of that death is but its termination, but the conclusion, as far as our feelings are concerned, of the process of dying. As soon as disease is established the act of dying is begun, and we have no reason to believe that, as far as the body is concerned, its nature is in any respect changed in what is called its termination. It is, from the first to the final ceasing of all the functions, a more rapid than natural decay of the powers of life, with, while sensibility lasts, more or less suffering, according to the cause which produces it. In recovery, our suffering terminates by the removal of that cause; in what is called death, by our becoming insensible to its effects; the bodily process being in no other way influenced by our total insensibility, to which the name of death is applied, but that the consequent ceasing of respiration accelerates it.

The body at this moment can no more be regarded as in the act of dying than at any other period of the disease; and the removal of the offending cause will not only in many cases at this period, if proper means be employed, but in some, even a short time after it, be followed by recovery. Thus, even after the

period at which, according to the common meaning of the word, the process of dying is completed, it is, under certain circumstances, not too late to arrest that process, and restore the sufferer to the perfect enjoyment of his faculties. Recovery may take place after respiration has, from submersion, for a few minutes ceased, and the sufferer is, in the common acceptation of the term, dead—his sensibility, and consequently his respiration, independently of artificial means, being finally extinguished.

That this may happen, it is necessary not only that the vital system should have been just before in a state of healthful vigour, but also that the respiration should not have failed from the failing sensibility, but the operation of the offending cause. Here the sensibility fails from the failure of respiration, not, as in other cases, the respiration from the failure of the sensibility; but this difference in the succession of events makes no difference in the general nature of the state induced.

The recovery depends on our being able, more or less perfectly, to restore the function, the failure of which has caused the failure of all the others, as far as it has taken place, before the process of dying has proceeded too far for the restoration of the sensitive system. If no artificial means are employed, the date of death here is the time at which the sensibility ceased, and justly, because at that time death, according to the common meaning of the word, has taken place. The individual no longer feels and wills.

If there were even the last remains of sensibility, breathing would take place without external aid, as happens when the submersion has not been long enough wholly to extinguish it. The individual has, without such aid, finally ceased to feel and will, and is therefore what we call dead. His blood still continues to move, and all the assimilating processes, as appears from the experiments above referred to, are still going on; but this is no more than happens, more or less, in all cases after what is called death; the only difference being, that from the nature of the offending cause, and the short duration of the disease, these

functions are in a state of greater vigour than when the loss of respiration has been the effect of the loss of sensibility, which makes no difference in the nature either of their remaining powers or the circumstances in which they are placed, and would not prevent their ceasing, as usual, if no means were employed to arrest the dying process. I have dwelt the longer on this case, because it affords a good illustration of some of the preceding as well as following parts of the subject.[1]

[1] From the experiments which have been laid before the Royal Society, (Philosophical Transactions for 1822, 1827, and 1829,) we have reason to believe that the effects of artificial respiration in restoring those whose breathing has been interrupted till the sensibility is destroyed, would be greatly aided by the use of voltaic electricity sent through the lungs in the direction of their nerves, and that many might thus be restored in whom inflation of the lungs alone fails. The inflation of the lungs in such cases acts in two ways. It gives to the blood of the smaller vessels of the lungs some of the arterial properties by which they are often excited, and acting through the blood of these vessels, it communicates to that of the larger vessels, and of the heart itself, more or less of the same properties, independently of the blood already changed being moved on towards this organ; for M. le Gallois has shown that after the circulation has permanently ceased, the blood may, to a certain degree, be changed by inflating the lungs, not only in the trunks of the pulmonary veins and the heart itself, but even in the great arteries.

There is reason to believe, from the whole of my experiments, that the lungs should not be inflated more than eight or ten times in the minute, and that the injection of large quantities of air and great force in its injection should be avoided, and consequently the patient placed in the position in which the chest expands with greatest ease.* One of the chief defects of artificial breathing is, that in it the chest is expanded by the

* If the air be thrown in more frequently or in greater quantity than the remaining powers of the lungs are capable of employing, it acts as a cooling process, and is highly injurious. One of the chief defects of artificial respiration arises from our not being able to ascertain either the precise quantity of air or the frequency of its injection required by the particular state of the circulating system in the lungs. We know that in the case before us the demand cannot be equal to what it is in health.

The approach of death, if we are aware of it, must always be more or less impressive, not only because we are about to undergo an unknown change, but are leaving all that has hitherto interested and been grateful to us. Even here, however, for the most part, the laws of our nature are merciful. Most diseases of continuance (for we shall find there are some exceptions) not only gradually impair our sensibility, but alter our tastes. They not only render us less sensible to all impressions, but less capa-

pressure of the injected air, whereas in natural breathing the air enters in consequence of its expansion. But the most essential difference between natural and artificial breathing in such circumstances is, that there cannot, till recovery is far advanced, be the proper supply of nervous influence, the due action of the vital parts of the brain and spinal marrow only being restored in proportion as the due force of circulation returns. Now it appears, from what is said in the Philosophical Transactions for 1822 and 1827, that voltaic electricity sent through the lungs in the direction of their nerves, is capable of performing, as perfectly as that influence itself, the part which belongs to it in respiration, which is so essential, that the more perfect animal always dies from impeded respiration if the nervous influence be withdrawn from the lungs, unless voltaic electricity be supplied, which we have seen enables it to breathe as well as when the nervous influence is entire.

A proper apparatus, therefore, for sending voltaic electricity through the lungs in the direction of their nerves, and in due power, should be added to the other means of resuscitation, which would render them, and probably to a great degree, more successful. The force of this observation will be perceived when it is considered that it is at the time of the first application of the remedies that the chance of recovery is greatest, and consequently that the immediate application of the whole means of healthy respiration, as far as we possess them, is of most consequence. It appears from what has been said, that the functions of respiration cannot be restored till the due degree of nervous influence is supplied, and this cannot happen from inflation of the lungs till the due force of circulation returns. The fact, explain it as we may, is, that voltaic electricity so perfectly supplies the place of the nervous influence in the lungs, that their functions are equally perfect under the influence of either. The one can only be supplied at an advanced period of recovery, that is, in fact, only in those cases where the success of our endeavours can be secured by

ble of enjoying as far as we are still sensible to them. The sight of a feast to a man who has lost his appetite is disgustful, and a similar change takes place in a greater or less degree with respect to all other means of enjoyment.

These circumstances constitute a great part of the difference of our feelings with respect to what, in common language, is called a violent and a natural death. In the latter, as far as the sensibility is impaired, we are more or less in the state of old age, and, in addition to this change, our tastes are perverted. By these means the relish for life is in a great degree destroyed before we lose it. Thus in disease, the most timid often meet death with composure, and sometimes, as I have repeatedly witnessed, with pleasure. I have even known the information that the danger was passed, received only with expressions of regret.

To the form of death I am considering, belongs a large proportion of the diseases of long standing, and whatever else tends gradually to exhaust the powers of the sensitive system, great mental excitement, too laborious a life, &c. The diseased state of the sensitive parts of the brain and spinal marrow, thus induced, spreading to the vital parts of those organs, terminates in a state of nervous apoplexy, the nature of which I had occasion to explain in the chapter on Sleep, and to contrast with that of apoplexy from compression, in the most unmixed cases of which the offending cause only producing a state analogous to the healthy exhaustion of the sensitive system, but greater in degree, its influence is throughout confined to that system.[1] In the former case we see all the vital functions deranged; in the latter

other means; the other is, in all cases, within our reach on the instant. As soon as the patient inspires, the use of the voltaic electricity should be discontinued.

[1] The reader has seen that simple and uniform pressure of the brain does not produce such a state of the vital parts of that organ as to derange the circulation, the effect of such pressure on the sensitive organs of the brain being of the same nature, as far as relates to the vital system, as the exhaustion occasioned by the exercise of their functions; which fur-

the breathing alone affected, except as far as its state affects the others, death arising merely from respiration ceasing in consequence of the loss of sensibility; and so exclusively is this sometimes the case, that I had occasion to refer to an instance in which the patient breathed only two or three times in the last ten minutes, but each time drew the air freely into the lungs; a proof that he died without any accumulation of phlegm there, and consequently without any disorder of the vital functions, but such as arose from the increasing insensibility.[1] Here the failing powers of the sensitive, affected the vital system in no other way than in sleep, the only difference being the degree in which the sensibility was impaired. Such cases are extremely rare. In by far the majority, from some inequality in the effects, or other peculiarity of the cause of pressure, at the same time that the sensibility is morbidly impaired, either a diseased state of a different kind is induced on the sensitive parts of the brain, which, as soon as established, begins to spread to the vital parts of that organ, or the cause of the disease itself more immediately affects the latter.

In the more rapid cases, the diseased state of the sentitive, which spreads to the vital parts of the brain and spinal marrow, supervenes without being preceded by a state of exhaustion, only differing from sleep in being greater in degree, in proportion as the cause which produces it is more powerful and protracted.

ther appears from the whole functions of health being immediately restored on the removal of the pressure, which only proves fatal by its continuance more and more impairing, and at length destroying, the sensibility. Many years ago, a man in whom the ossification of the skull had never been completed, exhibited himself in this country. By pressure made on the unossified part, he was immediately brought into a state of apoplexy, which always disappeared, leaving him wholly uninjured, on the removal of the pressure.

[1] It has been shown by many experiments, detailed in the Philosophical Transactions, that derangement of the assimilating functions is always attended with accumulation of phlegm in the lungs, this being the first indication of derangement of these functions in them.

The effects of diseased states of the sensitive on the vital parts of the brain and spinal marrow, differ according to the nature and degree of the offending cause. When they are such as in the first instance to produce a state analogous to sleep, their injurious effects are necessarily more or less gradual, the first operation of the agent differing only in degree from that of the usual stimulants of life; but where the offending cause is more powerful in degree, or of a more injurious nature, the stage of exhaustion is lost, and the immediate effect on the sensitive system is that species of debility which the vital parts of the brain and spinal marrow, having no power to relieve, partake of; and when the cause is both violent and sudden, its effects on these parts are often such as immediately to destroy the circulation.

The Experiments which have been laid before the reader, prove that although the heart and vessels do not derive their power from the brain and spinal marrow, it may be destroyed by impressions made on them. Thus it is that violent passions, either of a pleasurable or painful nature, in consequence of the sympathy which subsists between the sensitive and vital parts of these organs, have sometimes proved instantly fatal.

Here we have an effect from the causes of disease wholly different from that of the usual stimulants of life. The direct operation of the agent produces a state of debility in the sensitive system altogether of a different nature from that which constitutes the healthy exhaustion of sleep; and it will assist the memory and facilitate the means of reference to regard as the second form of what, for the sake of distinction, I call violent death, that which arises from all those causes which produce in the sensitive system this species of debility in the first instance, that is, debility without previous excitement, in whatever degree they have this effect, regarding, as the first species of such a death, the form of death we have been considering, that in which the cause, in the first instance, produces the stimulant effect, and consequently the exhaustion of sleep.

When the cause of the second form of violent death, according to this division of the subject, is extreme, no time is afforded for its less powerful effects to show themselves. When it is less violent, so that the circulation, though impaired, still goes on, we find all the vital functions impaired along with it. The assimilating processes are doubly assailed by the failing supply of nervous influence and the lessened powers of circulation. These effects, we have seen, may arise from the excess of the stimulant operation of agents, but they are not necessarily the consequence of any operation of this kind, but may be as much the direct effect of the agent as the stimulant effect itself. It is, the offending cause and state of body being the same, when the operation of that cause is most powerful, that its debilitating effect is most unmixed. In proportion as it is less powerful, the case partakes more of the nature of the form of death, in which the first effect of the offending cause is that of a stimulant.

This is readily explained. I have been at much pains in the preceding pages to point out that all agents capable of affecting the living animal, whether making their first impression on the mind or body, applied in a certain degree, act as stimulants; in a greater degree as sedatives: that is, as means of directly impairing the power of the part they act upon.[1] We know of no exception to this law, and the stimulant and sedative effects of different agents bear no particular proportion to each other; but the greater the stimulant power of the agent, it must be applied to the greater extent to produce the sedative that is the directly debilitating effect, and the greater its sedative power, in the smaller extent, to obtain from it the stimulant effect. The proportion which the stimulant and sedative effects of the same agent bear to

[1] See my observations on the term sedative in my Treatise on the Influence of Minute Doses of Mercury, which, from the want of some more appropriate term, I shall here employ for all agents which impair the power of the whole or any part of the animal frame without previous excitement.

each other is always the same, that is, its mode of application and the state of the body being the same, for the more gradual the application, the more the stimulant; the more sudden, the more the sedative effect prevails; and the less vigorous the functions, the less they are capable of the stimulant, and the more they are subject to the sedative effect. Thus torture, which produces sleep, that is, the exhaustion which is the effect of the stimulant operation, in the hardy savage, acts as a sedative in the less robust European. While the former sleeps the latter dies; and the more sudden its application the less the constitution is capable of resisting it.

The sedative effect, in whatever degree, is of a nature so different from the exhaustion which constitutes sleep, that its tendency always is to prevent the latter; and when the stimulant operation of the causes of disease exceeds that of the usual stimulants of life, and thus tends to the sedative effect, in the same proportion the tendency of these causes, although in the first instance to produce sleep proportioned to their stimulant effect, is eventually to prevent it. There is a degree of fatigue the repetition of which produces fever, not sleep.

Such being the principles on which all agents capable of affecting the living animal operate, we readily perceive why the more sudden and powerful the cause of disease, the more it inclines directly to produce a state of debility, and when it is most so, why this tendency is unmixed with any degree of the stimulant effect.

But it is not necessary, as appears from what is said of nervous apoplexy in my paper on Sleep, that the operation of the agent should be either violent or sudden, to produce, even in the first instance, more or less of the sedative effect, if to be of a nature suited to produce it. In proportion as its application is less powerful, however, its peculiar effects are necessarily so also. Instead of preventing the tendency to sleep, it only impairs it; and the morbid state of the brain and spinal marrow shows itself by symptoms which less immedi-

ately threaten life. The sedative effect of agents may exist in all possible degrees, from the effect of the rage and joy which has produced instant death, to that of the settled grief, which only in the course of years destroys its victim; from the pain of a scald so extensive as to produce death in a few minutes, to the irritations of confirmed indigestion, under which the patient often lingers for a great portion of life. Whether the effects be sudden or gradual, the tendency, in all such cases, is the same, to terminate in a state of general debility, that is nervous apoplexy, in which all the powers of the system are equally impaired.

The first impression of the cause is on the sensitive parts of our frame, which, without previous excitement proportioned to the debility which ensues, impairs their functions; and this debility, in consequence of the sympathy which exists between the sensitive and vital parts of the brain and spinal marrow, spreads to the latter, and thus the vital functions are, more or less quickly, so impaired that they can no longer maintain those of the sensitive system.

The nature of this death is well illustrated by the effects of severe accidents, many of which operate on the same principle as the scald. The effects of severe blows on the head and spine are very complicated. They at once impress equally the sensitive and vital system; but when the cause of injury is confined to less vital parts, as in the case of the scald, their first impression is on the sensitive system alone, or so nearly so, that the difference may be overlooked. Such was the cause of death in the case of the late Mr. Huskisson, with the circumstances of which the public are well acquainted; and hence it is that life is often saved by amputating a limb in which a cause of extreme irritation exists, that caused by the operation being more easily borne than the protracted irritation of a shattered limb, if the accident has not so subdued the strength that the additional irritation of the operation would itself prove immediately fatal.

To the same head belongs the death from the bite of rabid animals. The hydrophobia is a disease of the sensitive, spreading to the vital parts of the brain and spinal marrow: and such is the effect of many other poisons.

It is evident that the form of death I am now considering is of the same nature as the preceding, with the exception of the early stage of the latter. The sedative state of the sensitive organs is of the same nature, whether it has arisen from the excess of the stimulant operation, or from the more direct effect of the agent, when applied in such extent as at once to produce this state. The symptoms produced in the sensitive, and the manner in which they influence the vital system, are the same in both. The same observations, therefore, which apply to the latter stage of the first of these forms, apply, more or less, to the whole progress of that we are considering. In both, what is called death is the final extinction of the sensibility; the termination, as far as relates to our consciousness, of the process which has been going on from the first establishment of the disease. As sleep is the completion of the temporary and limited exhaustion of the excitability which has been going on during the day, death is here the completion of its absolute and final exhaustion, which has been going on during the disease; and it is evident, that as the sensibility decreases, the suffering must become less, and consequently that it is least of all at the moment of what we call death. These observations, however, we shall find do not apply, in the same extent, to the forms of death which still remain to be considered.

The three forms of death to which the attention has been directed in the preceding part of this paper, namely, that from old age, that from excessive stimulants acting on the sensitive parts of the brain and spinal marrow, and that from agents, also making their impression on those parts, applied to such extent as to act as sedatives, agree in an essential respect. The offend-

ing cause makes its impression on the organs of the sensitive system, and therefore in all, the sensibility is more or less directly impaired; and although it is only in the first that sleep can be regarded as the immediate cause of what is called death, the cause of injury in the second stage of the second form, and throughout the whole of the third form, producing the sedative effect, and consequently more or less tending to prevent sleep, yet tends, although in a different way, to impair the sensibility; and the termination in all such cases, as I have already had occasion to observe, if no other cause of injury arise in the course of the disease, is a state of nervous apoplexy, in so many cases the prelude of death, which, if not sufficiently violent or sudden, so to impair the powers of circulation as thus immediately to destroy those of the sensitive system, proves fatal by equally impairing the sensibility, and in consequence of the sympathy existing between the central organs of the sensitive and vital systems impeding the assimilating processes; and as sleep relieves us from the ordinary stimulants of the day, the insensibility thus induced, relieves us from the sufferings of the disease, which, although it is not, like sleep, preceded by the grateful feelings of repose, is preceded by a gradual diminution of those sufferings.

The forms of death which remain to be considered differ essentially from the foregoing. It will place in a clearer point of view both what I am about to say of these forms of it, and what has been said of its preceding forms, to consider more minutely than has hitherto been done in this paper, or, as far as I know, in any other discussion on the subject, the nature and relation of the functions of the living animal.

In the community of functions which constitutes the life of man and all the more perfect animals, the sensitive are the working functions, those by which we perceive and act; the vital, those by which they are maintained. To the former, therefore, belong

the immediate wear and tear of intercourse with the external world, and, consequently, the necessity of accommodating themselves to an infinite variety of circumstances. The vital functions, having but one object, pursue a steady course, from which, in health, they never deviate, except as far as is necessary to accommodate themselves to the necessities of the more eccentric functions of the sensitive system, the well-being of the organs of which depends on them; for they are capable of immediately influencing as well as being influenced by the inanimate agents which exist within our bodies; on the action of which, as appears from the various facts which have been laid before the reader, the due structure as well as functions of every part depend. On this principle our food is digested; on the same principle the heart beats, and the secreting and assimilating organs effect all their chemical changes. Thus the sensitive parts of the brain and spinal marrow are maintained, and thus also are maintained two sets of organs; through one of which, namely, the organs of the external senses with the nerves which convey the impressions made on them, those parts are capable of being influenced by the inanimate agents external to our bodies; and through the other of which, namely, the nerves and muscles of voluntary motion, they are capable of influencing those agents. These two sets of organs, allied by their vital properties to the sensitive parts of the brain and spinal marrow, and by their capability of being excited, and in their turn of influencing the inanimate agents of the world which surrounds us, form the links which connect and enable to conduce to one end the operations of the sensitive organs, namely, the immediate organs of the sensorial powers, and the operations of inanimate nature; two classes of operations which have nothing in common. Let us here pause to consider more particularly the positions stated in this paragraph.

However repugnant it may be to our preconceived opinions, we shall, I think, when the whole of the facts on the subject are carefully weighed, find it impossible to avoid the conclusion, that

all the vital functions, and all those functions of the sensitive system by which the sensorial powers influence or are influenced by the external world, are the results of inanimate agents acting on living parts, or living parts on them. Such, as far as I am capable of judging, must be the conclusion, if we compare the results of experiments, an account of which has been laid before the reader,[1] with observations too simple to require any illustration from experiment.

With regard to the first of the classes, the vital functions, it is evident that the functions of the alimentary canal are excited by the food, of the lungs by the air, and of the heart and blood-vessels by the stimulating contents of the blood.

The blood, as it circulates in the vessels, is justly said to be alive. It possesses properties essentially different from those of inanimate matter: but we know that it is not by its vital properties, which are bestowed on it for other purposes, that it stimulates the heart and vessels, because its stimulating contents, when separated from it, produce the same effects on them. The experiments relating to the maintenance of animal temperature point out one of the purposes answered by the vital properties of the blood, and all the experiments relating to secretion and the assimilating processes point out the other purposes of its vitality. It possesses vital properties, not for the purpose of acting on other parts, but for that of duly responding to the inanimate agent, which acts on it in all these processes; for that the secreting and assimilating processes depend on the action of an inanimate agent, appears from the experiments which prove that they depend on the nervous influence, which has been shown by direct experiment to be capable of its functions after it has been made to pass through other conductors than the nerves,[2] and therefore cannot have the properties of a vital power; to say

[1] See also Philosophical Transactions for 1817, 1822, 1827, and 1829.

[2] Philosophical Transactions for 1822, 1829, and 1833.

nothing of those experiments by which it has been shown that all its functions may be performed by an agent which operates in inanimate nature.[1]

With regard to those functions by which the intercourse of the sensitive parts of the brain and spinal marrow with the external world is maintained, it is evident that the organs of the external senses are excited by inanimate agents external to our bodies, and that the muscles of voluntary motion are capable of influencing those agents; and we know that those muscles are excited by the nerves, whose powers, as appears from the experiments just referred to, depend on an inanimate agent.

With respect to the nerves of sensation, by which impressions are conveyed to the sensorium, these form part of the sensorial system, and their functions are no farther allied to the operations of inanimate matter than that they are capable of conveying impressions made by it. We know from the nature of the phenomena that the only influence they convey, which is put in motion by the action of inanimate agents on the matter of which they are composed, is a vital power which acts in concert with the vital powers of the sensorium. Here it is at once evident that vital powers alone operate, all analogy with the properties of inanimate nature having disappeared.

Thus it appears that all the functions of the living animal, by which it is maintained, are the results of the agents of inanimate nature acting on organs composed of materials of the same inanimate nature with the agents which act in concert with them; the peculiarity of the results depending on those organs (in addition to the properties they have in common with the agents which excite them, which thus enables them to act in immediate concert with them, and from which arises the striking analogy they bear to the operations of inanimate nature) being endowed with the principle of life. The more internal functions, on which all our feelings and other mental powers, and consequently the regulation of our intercourse with the external world, depend,

[1] Philosophical Transactions for 1822, 1829, and 1833.

are suited to their ends by the simple arrangement of their organs operating by their vital properties alone; hence in these functions, the total loss of all analogy with the operations of inanimate nature.

It is evident that in such a system as I have been describing, there are two principles, either of which may determine the decay of all the sensitive functions. The organs of the functions by which the intercourse with the external world is maintained, may become incapable of their work, or those of the functions which maintain them, of their office. In the only natural death, that of old age, we have seen both these principles of decay in operation. The sensitive functions are gradually dimmed, and the vital functions gradually become less active.[1]

Life, without much violence done to language, has been called a forced state. It consists of excitable parts called into action by suitable stimulants. These stimulants, it appears from what has been said, are all of an inanimate nature, for although the sensorial organs can only be excited through the nerves associated with them, their action, it is evident, equally, though not so immediately, depends on the agents which excite those nerves. Hence the harmony which exists between the living powers of the animal body and the powers of inanimate nature. There is nothing in common in the nature of these powers; but our organs being composed of materials of the same nature with the world which surrounds us, are excited by the agents which operate in that world; and the parts thus excited being endowed with vital

[1] It is observed in my Inquiry into the Laws of the Vital Functions, that in the most sudden death arising from causes which instantly destroy the powers of the nervous system, all the vital powers are at once destroyed; but this is only to be understood comparatively. The time in such cases required for their destruction is short; but in all the instances I have witnessed, the same succession, however rapid, could be observed. It was still evident that the muscular and nervous survived the sensorial functions. After these functions had ceased, slight flutterings of the heart and fleeting contractions of the muscles of voluntary motion could still be observed.

properties, as well as those which enable them to respond to inanimate agents; those functions, which bear no analogy to the properties of such agents, are by their intervention effected.

These organs themselves are a part of inanimate nature. Deprived of their vital powers, they may still, as far as we see, be perfect in all their parts. On what their vital powers depend, we know not. In the study of these powers, and the relation they bear to the other powers of nature, we must be satisfied to take the facts as we find them. And what other knowledge have we of the inanimate powers themselves? Do we know more of the nature of gravitation or electricity than of life? It is the properties, not the essences, of things which are the objects of our senses. Our nature must be changed before the latter can be made a subject of inquiry. Life is a certain train of phenomena, depending on the peculiar state of its organs produced by the action of the same agents, which operate in other parts of nature, on the material organs of our frame. We may arrange these phenomena in the way that best assists the memory, and best shows their relation to each other and the other phenomena of nature; but no task can be more hopeless than the attempt to proceed one step further, either with respect to the living powers or any other principle of action. Such an attempt is beyond not merely the limits, but the nature of our minds. It is the blind attempting a knowledge of colours.

When we say we understand any of the phenomena of nature, we only mean that we are able to class them with other similar phenomena. We say that we know why a stone falls to the ground, because we class its fall with the other phenomena of gravitation. With regard to the phenomena of animal life, we at once see the limit of our inquiries, because it is self-evident that these phenomena exist nowhere but in the living animal, and consequently that there is no more general principle to which they can be referred, a position so evident that it is difficult to understand how it could ever have been overlooked.

It is customary to speak of life as a subject of peculiar mys-

tery. But if what has just been said be correct, we have precisely the same means of acquaintance with it as with the other powers of nature. Its phenomena are as open to observation and experiment as the phenomena of any of these powers; and we possess no information respecting any of them but such as is derived from those sources. The greater appearance of mystery arises, not from the greater obscurity of the nature of life, but from its phenomena bearing less analogy to those of the other powers of nature than these bear to each other; in consequence of which the former are less familiar objects of contemplation. Simple as such observations are, they cannot be regarded as superfluous, when we see them overlooked by such writers as Hartley, Hunter, and others of almost equal name.

We cannot be surprised that the inanimate agents, which are incapable of any change that unfits them for their office, should at length effect a permanent change in the vital parts on which they operate, of all parts of nature the most changeable. Hence the death of old age.

The sensorial functions we have seen fail first, because their organs are removed from the immediate action of the inanimate agents which still excite the organs to which they are directly applied. The stimulating parts of the blood are still present to excite the vessels, and the nervous influence, as appears from direct experiment, is still present to support the functions of assimilation and secretion; but the sensorial functions, being the results of vital parts acting on each other, as the vital powers fail, the powers of the parts acted on and those which act upon them failing together, these functions necessarily cease.

For the same reason, it is in the former, the organs of the nervous and muscular systems, that the decay begins. Their powers are gradually impaired by the operation of the inanimate agents which excite them, and the sensorial powers, as appears from all the phenomena of our decay, only fail in consequence of their failure; but as a certain vigour is necessary to render the latter capable of maintaining the sensorial functions, these for

this reason also necessarily cease before the total extinction of those which maintain them.

In the forms of violent death which have been considered, the offending cause makes its impression on the organs of the sensitive, in those which remain to be considered on the organs of the vital system.

It is evident from what has been said of the nature and relations of the functions of the living animal, that there is one class of the causes of death which is necessarily confined to the vital organs. On them, we have seen, the inanimate agents, on the operation of which life depends, make their impressions. Those which impress the organs of the sensitive system excite only the functions by which our intercourse with the external world is maintained, and consequently may cease to operate without at all endangering life. But the withdrawal of the agents which excite the vital organs as certainly proves fatal as the loss of power in these organs themselves.

The operation of such causes is too simple to require any comment. It is evident that the want of food must destroy the digestive and other assimilating functions, that of air the functions of the lungs, and the loss of the stimulant qualities of the blood, those of the heart and blood-vessels.

The other causes, which belong to the forms of death I am now to consider, operate in a manner analogous to the offending causes which make their impression on the organs of the sensitive system; for although the vital organs are not subject to the same species of exhaustion with those of the sensitive system,[1] like them they may be debilitated either by the excess of the stimulant, or the more direct effect of the agent, according to the degree in which it is applied. The usual excitement of fever terminates in debility of the heart and blood-vessels, or where the cause is more powerful, as we see in the worst forms of typhus,

[1] See the chapter *On the Nature of Sleep.*

it may directly impair their powers, that is, act as a directly debilitating power; and similar observations apply to the effects of the offending cause on all the other vital organs. Although such are uniformly its effects on the parts on which it operates, its effects on the system in general, in consequence of the sympathies of our frame, admit of greater variety. These also may be divided into two classes.

In considering the second of the forms of death in which the impression of the offending cause is confined to the organs of the sensitive system, it appeared that when it is both violent and sudden, it immediately, in consequence of the sympathy of the sensitive and vital parts of the brain and spinal marrow, and the influence of the latter on the heart and blood-vessels, destroys the circulation;[1] whereas, when less powerful, it proves fatal, not only more slowly, but also in a different way. A similar observation applies to the causes of death which make their impression on the vital organs; for the circumstance of their being more or less violent and sudden, or making their impression on an organ more or less immediately essential to life, not only renders their effects more or less sudden, but essentially influences their nature.

When the cause affects an organ immediately essential to life, and is of such power as at once to destróy its function, death, depending wholly on the loss of that function, may be instantaneous; but when the cause operates less rapidly, or affects organs less immediately essential to life, death is not only more protracted, but the various causes of continued irritation which attend derangement of the vital, influencing the state of the sensitive system, it often arises as much from the impression made indirectly on the organs of this system, as on those to which the cause is applied, and sometimes more so. Thus, any cause which suddenly destroys the function of the heart or lungs, at once proves fatal, and the cause of death is simply the loss of a func-

[1] Philosophical Transactions for 1815, and the first and second chapters of the present part of this volume.

tion immediately essential to life; but a loss of function in the intestines produces, not immediate death, but a series of causes of irritation, which exhaust the powers of the sensitive system, and death arises as much from this cause as from loss of function in the seat of the injury. Thus a blow on the stomach may instantly prove fatal by the impression it makes on the vital parts of the brain and spinal marrow, without producing any other cause of derangement;[1] but the inflammation of that organ, by the torture it occasions, often exhausts the powers of the sensitive system, before the inflammation has time to run the course that would prove fatal by its effects on the stomach itself.

We observe the same thing in a more remarkable degree where the organ is still less immediately essential to life, and the disease consequently is more protracted. It is in this way that stone in the bladder proves fatal. If such local mischief do not occur as disturbs the usual course of the disease, life terminates in the same way as from torture, only more slowly as the suffering is less severe and continued, that is, in a morbid debility of the powers of the sensitive system, more or less, according to circumstances, affecting the vital parts of the brain and spinal marrow, and the last symptoms, as in cases where the cause of the disease makes its first impression on the sensitive organs, are those of nervous apoplexy.

In this way death from causes of injury, making their impression on the vital organs, often approaches very nearly to the nature of the other forms of death which have been considered; and in almost all instances, with the exception of the most sudden, this is more or less the case; and consequently many of the observations made respecting the other forms of death, apply to the form I am now considering, particularly those relating to the gradual diminution of sensibility and perversion of taste which so generally precede, and more or less reconcile us to, death.

I have already had occasion to observe, that even in some protracted cases there is little of this tendency. This, of course,

[1] Philosophical Transactions for 1829.

is most apt to happen where the sensitive system is least affected, and therefore where the cause of injury makes its impression on vital organs of little sensibility—on the lungs, for example, organs of peculiarly dull feeling—a wise provision, for the air is so variously impregnated, and in so many ways which it is impossible to guard against, that, were their sensibiltiy acute, we should be exposed to constant causes of irritation.

It is probably from its being so little so, that, of all our organs, their sensibility is least apt to be increased by disease, the common effect of continued irritation. Those who have been troubled with carious teeth know how sensible the gums, parts of comparatively dull feeling, often become in disease. Even the most severe inflammation of the lungs may exist without pain, although the difficulty of breathing, cough, and fever, which attend it, sometimes exhaust the feelings as much as pain. In its more chronic forms, however, it is often but little distressing even in these ways; and I have seen a few cases of pulmonary consumption, in which the sensibility and relish of life continued so entire, long after the patient was sensible of his approaching end, as to produce a state of mind peculiarly distressing, differing but little from that of those who look forward to what is called a violent death. This, however, is rare. In all serious and particularly tedious illness there is generally sufficient bodily suffering and perversion of taste, more or less, to blunt the sensibility, and in some measure to wean the patient from the love of life ; and we generally find the grief and agitation on the part of the relatives, and on that of the patient a degree of indifference and composure, which those who have only experienced the feelings of healthful vigour are at a loss to comprehend. Even the dread of death at length prepares us for it. The feelings of the criminal who is hanged on the instant are those of horror ; of him who has languished in prison, of resignation.

But of whatever kind and degree the previous suffering may be, and by whatever cause produced, the last act of dying, in the common sense of the word, is still but the extinction of the sen-

sibility, and consequently the termination of all suffering; and, as might from their nature have been foretold, often so calm is this last act, in diseases of long continuance, that the most anxious observer often finds it impossible to ascertain the moment at which it takes place.

The circumstance which has given rise to our notions respecting the sufferings of our last moments is, that in certain diseases there is a convulsive action of the muscles at the time at which the sensibility is extinguished. But these are not acts of volition. The laws of our nature tell us that they are not the effects of suffering; and we never see in the patient any indication that he suffers. They are of the same nature with the convulsive motions of the epileptic, of which he is wholly unconscious. Were they indications of a struggle of feeling, necessarily connected with the last act of dying, as has been supposed, they would be a constant symptom; whereas they only occur under certain circumstances of the constitution or the disease. One of the least painful of violent deaths is that from loss of blood; yet here this struggle very uniformly attends the last act of dying, according to the common acceptation of the term; and it is evident that here the sensibility, in consequence of the failure of circulation, is almost extinguished before this involuntary action of the muscles takes place.[1]

It is generally supposed that the struggle of the criminal, after the drop falls, is the measure of his sufferings. The most

[1] It may appear at first view that our condition would have been improved had we not been endowed with the sensibility which often renders disease so great an evil; but in the same proportion as our ease would have thus been consulted, our danger would have been increased. It is by the quick sensibility of our frame that we are warned of a thousand dangers, and enabled to guard against them. Such is the imperfection of our present state, that we enjoy few advantages which have not occasionally their accompanying evils. But there is no instance but that of sleep, which is rather an imperfection than a positive evil, in which the evil necessarily exists: and thus we have reason to believe that the sum of enjoyment is the greatest of which that state admits. The species is protected at the expense of the individual.

vigorous necessarily suffer the most, because in them the sensibility is with most difficulty extinguished; but it is not uniformly in them that this struggle is greatest. We have reason to believe that it is little, if at all, connected with the feelings of the sufferer. All such convulsive motions are of the same nature with what is called *subsultus tendinum*, so apt to occur in fever, even while the sensibility is little, if at all, impaired; but which gives no uneasiness but what arises from the motions of the limbs it occasions.

The causes of disease under various circumstances must act more or less interruptedly. In some cases their operation wholly ceases, and is renewed at intervals, causing the disease to intermit. There is a principle in the animal body on which the cure of all diseases depends, termed by writers the *vis medicatrix*, in consequence of which the more immediate effects of the offending cause are followed by others which tend to counteract them. If the surface of the bowels, for example, be irritated, a more copious secretion of their fluids and an increase of the peristaltic motion are excited, by which the irritation is relieved, and the cause of injury expelled; and although there are few cases in which the operation of this power is so simple as in this instance, in all diseases its effects may more or less be observed, and a great part of the object of medical treatment, as far as the nature of the disease is understood, is to assist and regulate its operations.[1] We find even in those diseases which are of the most

[1] Here, as in other instances, that imperfection of our present state, which we have reason to believe inseparable from it, appears. Nature, for example, relieves inflammation sometimes by exciting discharges from the inflamed part, sometimes by the process of suppuration; but she still employs the same means, although the effusion or suppuration by which the inflammation is relieved, from the nature or situation of the part affected, generally proves fatal. Such is the case in croup, the disease termed internal water of the head, inflammation of many vital organs, &c. In these cases it is the object of the physician to remove the inflammation by artificial means, before it has time to run to such terminations. In other instances, as in some external inflammations, his object is to promote these operations of the *vis medicatrix*, as the least injurious way of removing the disease.

continued form, partly from its operation and partly from the cause of the disease acting more or less interruptedly, more or less evident remissions. Hence, and from a thousand accidental circumstances which influence the course of disease, and many of which it is impossible to trace, we find in diseases of continuance, that at one time the stimulant, at another the sedative, effect prevails. Thus the sufferer appears at one time to be sinking, and at another to revive, without our always being able to trace the cause of such variations. All this the complicated nature of the animal body, and the various ways in which it may be influenced, would lead us to expect. We might also be led to expect that it would sometimes happen that when the excitability is nearly exhausted, such a cause of excitement might under certain circumstances occur as would suddenly exhaust that which still remains, and thus, by causing a sudden but temporary revival, prove the prelude to death. Hence what is termed a lightening before death, on which so many superstitions have been founded. This is seldom strongly marked. That it occasionally is so, we have sufficient evidence, and that it should be so, is perfectly consistent with the laws of the animal economy; but it will appear from what has been said, that, like the convulsive motions I have been considering, it has no essential connexion with the act of dying, and is not the consequence, but the cause, of its immediate approach.

Before I proceed to the last part of the subject, namely, the order in which the nervous and muscular functions cease, on which a very few remarks will be sufficient, I shall shortly recapitulate the leading features of the different forms of death, without recurring to the other parts of the subject, which are too numerous to admit of recapitulation; and make such additional observations as the recapitulation suggests.

We have seen that the forms of death—for, as I have already had occasion to observe, the whole operation of the causes of decay in strict language constitutes the act of dying—may be arranged under five heads.

1. The only natural death, that from old age, where all the powers of life, in consequence of the operation of the agents which excite their organs, gradually decline, and death is only the last sleep, characterised by no peculiarity, in which these powers, partly from their own decay, and partly from the lessened sensibility increasing the difficulty of restoring the sensitive system, become incapable of this office, in consequence of which the individual awakes no more; for it is to be recollected that it is not in the commencement, but in the progress of the last sleep that what we call death takes place. In its commencement, we have seen, the sleeper may always be roused by stronger stimulants than those which preceded it.

All the other forms of death, it appears from what has been said, may be regarded as more or less violent, some adventitious cause disturbing the natural process. They were divided into two classes; in the one the offending cause makes its impression on the sensitive, in the other, on the vital organs. The former were divided into those cases in which the debility which precedes the total loss of sensibility arises from the excess of the stimulant operation of the offending cause, and those in which it is the direct effect of that cause; the latter, into those cases in which the vital powers fail in consequence of their organs being deprived of the stimulants which excite them; and those in which the offending cause makes its impression on these organs themselves, the power of which, analogous to the operation of the offending cause on the sensitive organs, is destroyed, either by the excess of its stimulant, or its more directly debilitating operation, according to the nature or degree of that cause. Thus are induced,

2. The death which in its nature most nearly resembles the death of old age, that from excessive exhaustion of the sensitive system, from the operation of stimulants of greater power than this system can bear, notwithstanding the intervals of such imperfect repose as their continued operation admits of, without the supervention of disease; which, not being capable of relief from the continued action of the vital parts of the brain and spinal marrow, by sympathy spreads to them, the affection of each sys-

tem increasing that of the other, till all the powers of the sensitive system are destroyed.

3. The death in which disease of the sensitive system arises, not from causes over-exciting, but directly debilitating it; the debility they produce being of the same nature with that from excessive excitement, and running the same course as in the second stage of the preceding form.

4. The death which arises from the privation of the natural stimulants of the organs of life; and lastly,

5. That which arises from diseased states of those organs, analogous to the states produced in the organs of the sensitive system by the causes which make their impression on them.

If the foregoing include all the modes of decay, the physiological nature of death, in its various forms, is referable to very simple principles. In the natural decay, the excitability of the organs of both the sensitive and vital systems is gradually impaired by stimulants, which, whether existing within our bodies or making their impression from without, belong to inanimate nature; for it is by the impression of such stimulants alone that the functions of life are maintained. In the different kinds of violent death, with the exception of the death which arises from a failure of the natural stimulants of the vital organs, which is comparatively rare and extremely simple in its nature, we find the excitability of one or both of these systems, or some parts of one or both of them, capable of influencing all the others, more quickly destroyed by the continued operation of causes which either stimulate beyond the limits of health, or, applied beyond the limits of their stimulant operation, destroy the powers of life, either by directly destroying the powers of the sensitive system, or depriving it of those powers by which it is maintained. All these causes, it is evident, tend to the same effect, the extinction of the sensibility, the immediate cause of which, therefore, exists in the sensitive parts of the brain and spinal marrow.

Thus it appears that, in every instance,—for it will be found, I

believe, that there is no case of death which may not be referred to one of the foregoing heads,—what is called death and the loss of sensibility are one and the same, and therefore that the last act of dying can in no instance be an act of suffering; and this we have seen confirmed by direct observation, as far as the observation of the bystander can confirm it; to which may be added the experience of the sufferer himself, because those who, from submersion or other similar causes, have passed that portion of the act of dying where suffering can alone take place, and who have, as above explained, been in the common sense of the word dead, and in consequence of the degree of vigour still remaining in the vital organs restored by inflating the lungs, declare that they had been sensible of no suffering but such as arises from a less degree of the same cause which in them had wholly extinguished sensibility: an observation well illustrated by the circumstance, that those who are restored by artificial respiration, and could not have returned to life without this aid, and those whose breathing not having been long enough suspended wholly to destroy the sensibility, and who consequently, although to all appearance equally insensible, in a short time after the cause is removed breathe spontaneously, give precisely the same account of their sufferings.

In those in whom the sensibility has been extinguished by submersion, it is in the first part of the process by which they recover, not in the last part of that by which they lose, it, that they suffer, which it is not difficult to explain.

In the latter the sensibility is almost lost before it is wholly so. The apoplectic who has still feeling enough to breathe, who may still be roused to remove the extreme cause of suffering which the want of a supply of air in the lungs occasions, may be insensible to all other causes of excitement; for, in proportion to the immediate importance of that supply, is the feeling which impels us to obtain it. The circumstance of the breathing, independently of artificial means, being finally lost, is a proof that the sensibility is wholly extinguished; and as its extinction in such a case must be more or less gradual, the capability of acute suf-

fering, it is evident, must be lost some time before the period at which the want of air in the lungs cannot even be felt.

In the act of recovery, on the other hand, the sensibility necessarily begins to revive before the vital organs perfectly recover their functions after so severe a shock. The sensitive, on its revival, thus finds the vital system still more or less in a state of disease, to which the former, as its powers increase, is every moment becoming more sensible; for while the powers of both remain, all derangement of the vital is felt by the sensitive system; a wise provision, by which we are warned to guard against causes of danger confined to the former.

It will readily occur to the reader, from what has been said, that under certain circumstances more than one of the preceding forms of death may concur. The first indeed, the death of old age, may be regarded as so far a combination of more than one of the other forms, that the cause makes its impression on both the sensitive and vital systems; but its effects on both, as appears from all that has been said, are essentially different from those of disease.

In certain cases the cause of disease makes its impression on both systems, and then more than one of the last four forms concur. This, I have already had occasion to point out, necessarily happens from mechanical injury of considerable portions either of the brain or spinal marrow. When both systems are directly impressed by the cause of the disease, which is comparatively rare, it produces, as follows from what has been said, a combination of the third and fifth, or second and fifth forms, according as its effects are more or less sudden and severe.

Such in different cases is the varied course of our decay previous to the moment at which the sensibility is extinguished, emphatically called death, becauses it completes the decay of the sensorial powers, and leaves us only those that we possess in common with the vegetable world, for the vegetable, like the

animal, can convey its juices, form its secreted fluids, and in some instances move its limbs, if proper stimulants be applied; an additional argument, it might be shown, if any were required, for all such functions being the effects of inanimate agents acting on living parts.

After the removal of the sensorial functions, none remain to us but such as are maintained by the immediate action of those agents. Our bodies are hastening to be mingled with the matter of inanimate nature. They retain only those powers which immediately depend on its agents, and these are rapidly failing, because, for reasons which have been pointed out at length, the due application of those agents in the more perfect animals cannot long survive the loss of the sensorial powers.

The power of organising the elements of inanimate nature belongs, and some have supposed exclusively, to the vegetable world; but as we see plants, the mushroom tribe, possessed of no organising power, and therefore, like animals, nourished only by matter already organised, some of the lower species of animals, on the other hand, seem to possess this power. Thus, it would appear that there is a class of animals and of plants in which the animal and vegetable, in this essential respect, exchange their natures. As the animal becomes imperfect, and approaches the nature of the vegetable, the sensorial powers dwindle, and the lowest animals appear to extract their nutriment from air and water, which, being generally diffused, are at hand, and consequently obtained without any sensible effort on the part of the animal. His life, therefore, although not independent of the external world, is, like that of the vegetable, independent of any act of volition. As we rise in the scale of animals, the sensorial powers increase, and, in the same proportion, become more essential to existence. From those animals which obtain food without any act of volition, we come to those who can only obtain it by such an act, but who still, without any act of this kind obtain, the influence of the air, yet more immediately necessary to their existence. We arrive at length at the most perfect class, which can

neither obtain food nor air, except by an act of the sensorium. In them the sensorial power is as necessary for the inhalation of air as the ingestion of food. When sensation ceases, they as certainly cease to breathe as they cease to eat. Thus it is that in this class of animals the due application of the inanimate agents on which life depends, cannot long survive the loss of the sensorial functions.

AS we have been enabled, by the aid of the experiments referred to in the foregoing paper, to trace the steps by which the sensibility in the various forms of death is extinguished, that is, of our decay up to that moment which has for very evident reasons obtained the name of death; by the same means we may with more ease trace the steps by which the remaining powers of life are extinguished.

AS the powers of life fail, we have seen, the first functions which cease are those which wholly depend on these powers. The others, being the results of inanimate agents acting on vital parts, continue as long as those agents are supplied, for the purpose of exciting their organs. The first of these powers which fails is evidently the power of the capillary vessels, because their function continues as long as any blood can be supplied to them from the larger arteries. The circumstance of the action of the capillaries only ceasing when the larger arteries are empty affords a proof that the assimilating processes, without which their power would fail, are still more or less in an active state. These processes, we have seen, are immediately dependent on the vital parts of the brain and spinal marrow. The due mechanism of every part, it appears from direct experiment, depends on the action on the blood of the agent they supply. When the capillaries can no longer supply the blood on which it acts, it is evident that the functions of this agent must cease, and consequently that those parts of the brain and spinal marrow by which it is supplied, being thus deranged, their powers must cease also.[1]

In the first of any of the more perfect animals, unless the nervous

These are the last of the powers of life which fail, and thus the body of the more perfect animal is left subject to the laws of inanimate matter. The first functions which cease, are those of the sensitive parts of the brain and spinal marrow; the last, those of the vital parts of these organs.

CHAPTER XXII.

On the means by which the Frame of the more perfect Animal is formed into a whole.

We have now considered individually the various powers of the more perfect living animal. We have found in it, beside the mechanical powers which, it will be admitted on all hands, it evidently possesses in common with inanimate nature, four distinct

influence be supplied from without, the rudiments of the organs which supply it and those of the sanguiferous system must have been simultaneous creations, because neither is capable of producing the other, the functions of each being inseparable from those of the other. But we have seen that it is a necessary inference from direct experiment, that while the vital principle is unimpaired, the powers of circulation, provided the blood be duly exposed to the influence of the air, are, with the aid of voltaic electricity, capable of all the assimilating functions. No other powers are required for the maintenance and growth of the animal body.

We have reason to believe that the vital parts of the brain and spinal marrow may, like the lungs, be inactive in the fœtal state, some other means in this state being employed to supply an agent, which after birth can only be supplied by them. Well-grown fœtuses, perfect in all their parts, have been born without either brain or spinal marrow. The growth of such fœtuses must depend on the same causes as the growth of other monstrous productions in the uterus, namely, as far as relates to the brain and spinal marrow, on the powers of the mother alone, how applied it is at present impossible for us to say.

powers; three of them vital powers, properly so called, that is, powers having properties essentially different from those of the agents which operate in inanimate nature. In the fourth alone we recognise one of those agents; for we find it can exist in other textures than those to which it belongs in the living animal, and that we can substitute for it one of the powers of inanimate nature, without deranging the functions of life.

All these powers are employed, although in a very different way, in the construction of two systems in a great degree distinct; the end of the one being the maintenance of our bodies, of the other, our intercourse with the world which surrounds us.

I am now to consider the various relations those powers bear to each other in the maintenance of the foregoing systems; and the way in which these systems themselves are so related, as to form the animal body into a whole, in which no part can be affected without tending more or less to influence every other.

In order to ascertain the seat of the power on which muscular contractility depends, it was necessary, early in the present part of this volume, to enter on the relation which subsists between the muscular and nervous systems; and it appears from what is there said, that the nervous influence, whether in its effects on the muscles of voluntary or involuntary motion, stands only in the relation of a stimulus or directly debilitating power to the muscular fibre, according to the degree in which it is applied, and the manner in which the musles are impressed; a result, I may observe in passing, peculiarly in accordance with all the other facts which have been stated respecting the nature of that influence, because the same observation, we shall find, applies to all the agents of inanimate nature which are capable of influencing the muscular fibre.

In like manner, in considering the nature of the nervous system, properly so called, it was necessary to point out the independence of its powers: we found them all surviving the existence of the only power, the sensorial, on which they could be sup-

posed to depend; but at the same time, while the structure of our frame is entire, in all their functions, under its immediate control.

The relation which next demands our attention, is that which subsists between the organs of the nervous influence and the living blood.

The first thing which here strikes us is, that the blood-vessels and nerves uniformly accompany each other; from which we are led to infer that they co-operate in functions of very general necessity.

All the powers of the nervous system properly so called, we have seen, are chemical powers. Of this nature, therefore, must be all the processes in which they immediately co-operate. It is evident that where such powers are employed, to render them efficient, materials must be provided on which they may operate, and there must also of course be means by which these materials are duly exposed to their action.

The materials we find in the blood, the means, employed for the purpose of duly exposing them to the action of the nervous influence, in the capillary vessels, on which the minute extremities of the nerves (which we know, from numberless observations, are those parts of the nervous system by which its powers are immediately applied in the functions of secretion and assimilation, as well as the excitement of the muscular fibre) are distributed. As the central are the only parts of the nervous system properly so called, employed in the formation of the nervous influence, the extremities of the nerves are the only immediate organs of its powers in all its functions.

The motion of the fluids in the capillary vessels, as appears from experiments which have been laid before the reader, depends on a power which resides in themselves, in no degree depending on the power of the heart or arteries, except as far as is necessary for the due supply of blood to the latter, which form the reservoirs from which the capillary vessels draw their supply. When in the newly-dead animal a ligature is thrown round all

the vessels attached to the heart and this organ is removed, the motion of the blood in the capillaries, we have seen, continues unimpaired, and only fails in proportion as the supply from the large arteries fails.

By such means the materials on which the nervous influence operates are supplied and presented to it; and the means of supply, namely, the power of the heart and arteries, as well as that of the capillary vessels themselves, being, as we have seen, under the immediate influence of the same power which effects the chemical changes, the supply is proportioned to the demand under the various conditions of the ever-changing functions; and under the same influence are the means of removal, whether of secreted fluids or parts become unfit for the purposes of life.

Such are the nature and functions of the nervous power, and its relations to the muscular power and the powers of the living blood. When we turn to the sensorial system, we find it of a wholly different nature, and consequently the relations it bears to the other powers of the more perfect animal, wholly different. The immediate organs of the sensorial power, we have seen, are as it were hedged in and defended from contact with any of the agents of inanimate nature.

On the one hand, we find the nerves of sensation, which so far partake of the nature of the external world, that they are capable of receiving and propagating impressions from its agents, but in all other respects are allied to the organs with which they are associated.

The influence they convey possesses no property but that of exciting the immediate organs of the sensorial powers, effecting, in concert with them, results which bear no analogy to any of the properties of inanimate nature; proving that it is by the vital properties alone of the agents employed that the results are effected.

Thus the sensorial organs receive all their impressions, whether originating from without or within our own bodies.

On the other hand,—that is, that the sensorial organs may, without contact with any of the agents of the external world, impress those agents,—a more complicated machinery is required. The various nerves of sensation are the only means required for conveying impressions to these organs; but so simple an apparatus is not sufficient to convey to, and impress on, the materials of the external world, the dictates of volition.

The powers of the nervous system are here called into operation by the sensorial powers. The nervous, influenced by the powers of the sensorial organs, supply a certain set of nerves with the stimulus which excites the muscles of voluntary motion, the immediate agents by which the materials of the external world are impressed.

The great variety of the phenomena of life is one cause of their apparent obscurity. Such is their variety, that we are at first view lost in attempting any arrangement or even enumeration of them. An essential step towards their arrangement, as appears from what has been said, is their division into those which are the immediate results of the co-operation of the powers of life with those of inanimate nature; and those which have no immediate dependence on the latter powers; for all our functions mediately or immediately depend on the operations of the agents of inanimate nature. All are more or less directly excited by impressions originating in their agency.

The most purely sensorial functions, our pleasures and pains, are as dependent, though more remotely, on the excitement maintained by them, as the functions of the organs immediately impressed by them. Have not the excitements of memory as much originated in their impressions, as their more direct effects on the part impressed? And when the nature of our bodies, and the circumstances in which we are placed, are duly considered, what other result could be expected? Our organs, being composed of the same materials as the world which surrounds us, can only be directly influenced by agents of their own nature; and from that

world, and by the medium of those organs, all the materials, not only of our acquired knowledge,[1] but of our enjoyments and our sufferings, are derived.

And as, on the one hand, all our functions are more or less immediately excited by impressions made by the agents of the external world on organs composed of materials of their own nature, on the other we have no power of influencing them but through similar means. The only means of exciting our mental functions are the impressions of those agents on the organs of sense, and our only means of operating beyond our own bodies are through our organs of motion. Even when by our mental powers we influence those of other sentient beings, it is as much, though not so directly, by impressing the agents of the external world by the latter organs, as when we raise a weight or throw a stone.

Such is the general outline of the vital and sensitive systems, and the manner in which the various powers of the living animal are related in the formation of these systems. By the foregoing means, the nervous power maintains the vital functions, properly so called; and the sensorial power is brought to co-operate with the powers of inanimate nature—powers which have no properties in common.

The vital and sensitive systems obey very different laws, the difference depending on the vast difference in the nature of the sensorial and nervous powers, the leading powers which pervade all their departments, and to which all their other powers are subservient.

These other powers, it appears from what has been said, are the same in both, namely, the muscular power and the powers of the living blood, and, in the sensitive system, the nervous power itself; for in this system all the other powers of the living animal

[1] We are born with the knowledge which is immediately essential to our existence. See the chapter on the Nature of Death.

are directly subjected to the sensorial power; while, although we have seen the sensorial power contributing to the powers of the vital system, none of the powers of that system have any direct influence on it, their influence on the sensorial power being alone through the medium of its organs, the structure and well-being of which immediately depend on the vital powers.

In other respects also the laws of the two systems essentially differ. Nor will these differences surprise us, when it is recollected, as appears from the facts which have been stated, that while the leading power in the vital system is one of those powers which operate in the external world, that of the sensitive system not only possesses no properties in common with the agents of inanimate nature, but depends on a set of organs unapproachable in their healthy functions by any such agents.

When the facts adduced in the chapters on the Nature of Sleep and Death are duly considered, it will appear that a principal cause of difference in the laws of these systems depends on the difference of the laws of excitability in the organs of their leading powers. In those of the leading power of the sensitive system, all degrees of excitement are followed by a rapid proportional exhaustion of excitability; so that the effect of the usual stimulants of life for a few hours, renders a state of inactivity essential to the maintenance of their health; while the exhaustion of the excitability of the organs of the leading power in the vital system by those stimulants, is the operation of many times as many years, the one determining the recurrence of sleep, the other the natural duration of life.

Thus it is that those in whom, from habits of dissipation, extreme labour, or other causes, the excitability of the vital system is to a certain degree exhausted, but who, as they approach middle life, cease to be exposed to such causes, and during that portion of life, that is, from thirty to fifty or fifty-five, feel little inconvenience from the effects of their early habits, there still being in the vital system sufficient excitability for the usual functions of life; after this period, when the defect of excitability begins to be felt sooner or later by all, feel the effects of its

expenditure which had been so profuse in early life; many striking instances of which I have witnessed. Similar observations apply to long-protracted illness, severe misfortunes, or any other cause which at any period of life in a great degree, and for a considerable length of time, tend to exhaust the excitability of the vital system, although for a certain time the individual may enjoy his usual health after such causes have ceased to operate.

Thus it is that in almost all cases of great longevity we find that there has been little exposure during life to powerful causes of exhaustion of either body or mind; for we have seen that the nervous is immediately under the influence of the sensorial power; and that such instances are most frequent in the colder of the temperate climates,—heat, on the one hand, tending to exhaust excitability, and extreme cold, on the other, to render us less capable of excitement.

While considering the laws of excitability, it is necessary to bear in mind an essential property of all those agents which are capable of calling it into action, and which has demanded less attention than its great importance in the treatment of disease demands. There is no agent capable of influencing either of the two systems into which the functions of the living animal arrange themselves, whether it be such as makes its chief impression on the mind or body, which is not capable of acting either as a stimulating or directly debilitating power, according to the degree in which it is applied. There is none of any considerable activity which may not be applied in so small a degree as to act as a stimulant, and in so great a degree as to act as a directly debilitating power. The most depressing passion in a comparatively small degree will excite, the most exciting in an excessive degree directly debilitate; and the same stimulus by which either the nervous or muscular fibre is directly excited, will by its excessive application directly deprive it of power. I know of no exception to this law. All medicines within their stimulant range excite, and unless the excitement exceeds the degree which pro-

duces no correspondent depression, (for such a degree of excitement is compatible with the laws of the vital though not with those of the sensitive system,) it acts as a permanent tonic. All, beyond their stimulant range, act as directly, and although within that range, if of a certain intensity, as indirectly debilitating powers with respect to both systems.

It is evident from many facts, stated in the chapters on the Nature of Sleep and Death, that each of the foregoing systems is a whole, which cannot be influenced in any one part without a tendency to be affected in all others; a property which perhaps, more than any other, influences the progress of their deviations from the healthy state; for every part more or less feeling the change effected in any one, if there be any from accidental causes more liable to disease than the rest, this part particularly feels the cause which operates on all; and, as I shall soon have occasion to point out more particularly, is even the means of diverting its effects from every other part. Thus it is that diseases of continuance become complicated, and that an affection, attended with little risk in the part first impressed by the offending cause, often becomes formidable by its secondary effects.

The power which operates here, has been termed the sympathy of parts, the effects of which I shall have occasion to consider at length in the remaining, the practical, part of this volume.

From the facts which have been laid before the reader, we easily perceive the means by which every part of each of the foregoing systems is capable of influencing every other. Each is regulated by a leading principle, and in consequence of this, under an influence by which the affection of any one part tends to affect all others; because as all parts of each system both influence this principle and are influenced by it, it necessarily follows that all must, through it,—that is, through the central organs of each system, which alone are the immediate organs of its leading principle,—feel the affections of each. Such, together with the laws I am now to consider, is the source of the function to

which the term sympathy has been applied, the operation and effects of which we are soon to consider.

As each of the preceding systems is formed into a whole by its leading principle, the relations which these systems bear to each other have a similar effect with respect to the whole frame; for the affection of any one of its parts tends more or less, though much less powerfully than in the individual systems, to influence all others. The means by which the relation between the sensitive and vital systems, and consequently the most complicated functions, are maintained, we are here to consider; to some of them I have already had occasion to refer.

We have seen that the nervous power properly so called, the leading power in the vital system, is immediately under the influence of the sensorial power, the leading power in the sensitive system, and constitutes the medium through which all that part of our intercourse with the external world, by which the latter power influences it, is maintained. This therefore is the first bond of connexion to which I shall refer between the sensitive and vital systems. The second is the means by which the organs of both systems are maintained; for, as I have already had occasion to observe, the sensorial has a dependence on the vital system for the maintenance of its organs, as the vital, we have seen, has a more remote dependence on the sensorial system, for the maintenance of its organs; the connexion thus established between them being increased by both systems equally depending for the maintenance of their organs on the muscular power and the powers of the living blood; both of which are in their turn subjected to the nervous, and the former certainly, and the latter, we have reason to believe, through the nervous, also to the sensorial power.

The sympathy which prevails through all parts of each system also contributes to the influence of these systems themselves on each other; because the state of the parts secondarily affected in consequence of the power of sympathy, more or less influences

both systems, all parts being more or less supplied with nerves from both.

But we have sufficient evidence in the phenomena of disease, compared with the results of the experiments referred to, that here, as in the instances just pointed out, the central organs of the sensitive directly influence those of the vital system. A sympathetic pain, it is well known, referred to any part, will at length produce actual inflammation of the part. Now, while the pain alone exists, we know that the derangement, which produces it, is in the central organs alone of the sensitive system, and in no degree in the part to which it is referred; and we also know, from the facts which have been stated, that there is no channel through which this derangement can influence either the nerves or vessels of the part, but through the central organs of the vital system.

When the affection of the nerves or vessels of the part is the original disease, it influences the central organs of both systems by the actual disease of the part; but in the former case there is no other channel of communication than that just referred to. The central organs of the sensitive, having no direct power over either the vital nerves or vessels, can only influence them through the central organs of the vital system. Thus arises a double bond of connexion between the two systems, the central organs of the sensitive, directly influencing those of the vital system; and the nerves of the sensitive system being necessarily influenced by all deviations from a state of health in whatever part, and all parts being subject to the central organs of the vital system; the degree to which the effect in the sensitive system takes place being proportioned to that in which the part is supplied with nerves of sensation. As the central organs of the sensitive, directly influence those of the vital system, the latter, through the extremities of the different nerves with which the two sets of organs are associated, influence the former. Hence we shall find the fatal obscurity of many diseases of those vital organs, which are ill supplied with nerves of sensation.

To these means of influence on each other of the central organs of the two systems, must be added the mutual sympathy which exists between them from a law which has no exceptions in our frame, and which I shall soon have occasion to illustrate more particularly, that all neighbouring parts sympathise.

Different parts of the central organs of the sensitive system correspond to different parts of the general frame. This is perhaps sufficiently proved by our being enabled by experience to refer our sensations to the seat of the cause which excites them; but in many of the inferior animals, where both the brain and spinal marrow partake of the organs of the sensorial power, it may be proved by direct experiment, because after the removal of the brain we find the sensorial power lost only in those parts which derive their nerves from that organ.

But how comes it that the central organs of the vital system also have relation to certain parts of the general frame, the nerves associated with these organs conveying, as appears from what has been said, their combined influence, which is bestowed alike on all vital organs?

It is a law of the animal economy, amply illustrated by the phenomena of disease, that when an impression influencing the system generally is, by previous debility or any other cause, directed to a particular part, its operation is diverted from all others. Now it appears from a thousand phenomena that the suffering of the sensitive system, referred to any particular part, is sufficient, under certain circumstances, in consequence of the influence of the central organs of the sensitive over those of the vital system, to direct to it the effects of derangement excited in the latter. Thus even a diseased organ will often regain its healthy state, when the disease has spread to another, particularly if in the latter it takes deeper root, if I may use the expression. It is a daily occurrence for a disease of function to be finally removed by a disease of structure being established in another organ. Hence the good effects of artificially exciting disease in external parts, to relieve those more immediately essential to life;

and the still more salutary effect, when the laws of our frame themselves produce the same effect, because here it is the uninfluenced result of those laws, whereas in the former case their tendency is constrained by artificial means. Thus, for example, it is that the inflammation of a gouty joint or other external disease often relieves the derangement of a vital organ, and that artificially repelling this effort of the constitution to save a vital part, has so often proved fatal.

On the facts that the central organs of the vital system directly influence the functions both of the vital nerves and of the vessels of every part, while those of the sensitive system have no direct influence on either, many of the phenomena of disease depend; because it is only in proportion as these nerves and the vessels of the part are influenced, that any disease of the part itself exists, and consequently that there is any tendency to derangement either of function or structure in the part; of function alone if the nerves alone are affected, of structure also as soon as the vessels partake of the disease. This subject it will be necessary to consider at length in the last part of this Treatise on which we are now about to enter.

Such, together with the function of respiration, the nature of which, and the manner in which it forms a bond of union between the two systems, I have already had occasion to consider at length, are the means by which the frame of the more perfect animal is formed into a whole, and the function of sympathy and its other more complicated functions, above enumerated, effected. A powerful connexion is established among all parts of each of the systems into which the functions arrange themselves, depending on each being regulated by a leading power which influences every part of the system to which it belongs, and in its turn is influenced by every part of it: and these systems themselves, we have just seen, are intimately related in consequence of the nervous, the leading power in the vital system, by means of the control which the sensorial power exercises over it, being employed in the

accomplishment of many of the sensitive functions, and the sensorial power, the leading power in the sensitive system, in one of the most important of the vital functions; by the mutual sympathy which exists between the central organs of the two systems in consequence of their vicinity; by both systems not only depending for the maintenance of their organs on the same powers, but more or less directly on each other; by the powers common to both systems being under the influence of the leading powers of both; and by all affections of whatever part, whether original or sympathetic, necessarily influencing both its sensitive and vital nerves, and consequently the central organs of the systems to which they belong.

PART III.

THE APPLICATION OF THE INFERENCES FROM THE FOREGOING INVESTIGATION, WITH A VIEW TO IMPROVE OUR KNOWLEDGE OF THE NATURE AND TREATMENT OF DISEASE

INTRODUCTION.

It is observed in the preface that the defects in the practical department of our profession which the preceding Investigation tends to supply, are

1. Our not being sufficiently aware, from a defective knowledge of the relation the various functions bear to each other, of the tendencies of continued states of chronic disease; in consequence of which, in a numerous class of such diseases, the curative stages are often allowed to pass disregarded.

2. The attention in many acute diseases, from the same cause, being often confined to the more evident train of symptoms, where the derangement has had its origin in a more obscure affection, without the removal of which that of the more prominent disease is impossible; and,

3. From our not having been aware either of the nature or most important functions of the nervous influence or the seat of the organs on which these functions immediately depend, we have neither been aware of many of the symptoms which indicate the failure of that influence, nor in possession of the most effectual means of remedying its effects.

A little reflection at once points out that a mistake with respect to the seat of the organs of any important vital function,

or set of functions, the nature of the powers which maintain them, the laws they obey, and the relations they bear to each other, must necessarily tend to obscure our knowledge both of the nature of disease and the effects of our remedies.

It will be generally admitted that, except as far as it depends on the simple principle of employing the means, which had been found useful in similar cases, the practical department of our profession rests wholly on our knowledge of anatomy and physiology, a knowledge of the structure and functions of the animal body.

From the first of these three sources (independent as it is of previous information, and exercising only the simplest faculties of the mind) all practical knowledge of the healing art possessed in the rudest state of society is derived. As the limited nature and uncertainty of such a source appear, we are led to the means of assisting it.

We by degrees perceive the necessity of distinguishing the local and general, and the more immediate and remote effects of our remedies; and soon find that to make any considerable progress in such investigations, some knowledge of the structure and functions of the subject of our practice is necessary; and that in proportion as this knowledge is acquired, our means of relief become more effective.

When I entered on the preceding investigation, all our doctrines, as far as respects the general laws of our frame, it appears from what is said in the first part of this Treatise, were involved in a confusion which none of the facts, of which we were in possession, enabled us to unravel. I therefore began by repeating many of the experiments of the physiologists who had for their object the elucidation of those laws, at the head of whom I found Haller, Le Gallois, and Hunter. I found the results of their experiments such as they state them to be; but the inconsistency of their inferences left us no room to doubt that some of them are fallacious; which will not surprise us when we consider the state in which they found the subject: for anything that had

been done respecting the general laws of the animal economy before their time was comparatively of little account, being in most instances either so evident as not to require the aid of experiment, or erroneous.

It had, however, the merit of being to a certain degree consistent—a consistency not difficult of attainment—because, as the test of direct experiment had been little resorted to, it was easy to make any supposition which was necessary towards attaining it; but after the time of Haller, who may be regarded as among the first of those physiologists who applied with any accuracy to physiological inquiries, the principles of inductive reasoning, the dreams of the older physiologists began to share the fate they so justly deserved, and the consistency of our systems immediately suffered.

The few facts at first ascertained were stubborn things, and would not conform to the loose and fanciful theories which had prevailed. Thus difficulties arose, and, even so lately as at the time my labours commenced, prevailed, we have seen, to so great a degree, and caused such confusion in our doctrines, that some despaired of seeing it effectually removed.

The subject of the present part of this volume naturally divides itself into the diseases which originate in the sanguiferous, and those which originate in the nervous system; this term being understood in its most extended sense, that is, as including the organs of the sensorial and nervous powers properly so called.

The former class is simple, comprehending but few diseases, but some of those equally important from their nature, their frequency, and their intimate connexion with a large proportion of almost all other diseases; the latter so complicated, and of such variety, that, beside the diseases which depend wholly on the state of the nervous system itself, they comprehend, we shall find, certain modifications of the diseases of all our organs.

It is evident from the most cursory review of the diseases of the sanguiferous and nervous systems, that although those of the

latter partake more immediately of the affections of the sanguiferous, than those of the sanguiferous of the affections of the nervous system, the effects of derangement of the latter on the former system are more numerous, obscure, and difficult of treatment; the cause of both of which may easily be perceived, on comparing the results arrived at in the preceding part of this volume. The circulation, we have always known, is immediately necessary to the functions of the brain and spinal marrow, while it appears from the experiments which have been detailed, that those of the heart and blood-vessels may continue for a certain length of time after the former organs have ceased to exist; having on them only an indirect, but an exceedingly complicated dependence, through the functions of respiration, secretion, and assimilation.

CHAPTER I.

On the Diseases of the Sanguiferous System.

Of the diseases of the sanguiferous system there are some in which the force of the circulation is diminished, so that the supply of blood to the parts particularly affected fails, producing various symptoms according to the degree in which this takes place, and the nature of the organs chiefly influenced by it; and others in which the vessels of certain parts are distended with more than their due proporti on of blood, either in consequence of the increased action of the heart and larger arteries, or of the vessels of the part being so far weakened that their power of resistance is not in due proportion to the usual impetus of the blood. The two last states produce nearly the same train of symptoms, except that the former is preceded by the symptoms of a morbidly increased impetus of the blood throughout the system; but no diseases can differ more than those which depend on the seat of

this distension. These last are the diseases of the sanguiferous system, which here chiefly demand attention, both from their importance, and their being those most immediately connected with the results of the preceding investigation, namely, inflammation and congestion. To these, therefore, I beg leave to direct the reader's attention.

SECTION I.

On Inflammation.

INFLAMMATION may be regarded as the most important of all diseases, whether we look to its frequency, its severity, its relations, or its consequences. In its more chronic forms, it is the precursor of gradual disorganisation; in its acute forms, of those species of disorganisation which are most rapidly fatal. Its relation to other diseases is the most extensive, and its influence on their course the most important—a position illustrated by many of the diseases to which I shall have occasion to refer; for, in a large proportion of our most formidable deviations from a state of health, the obstinacy and danger are proportioned to the degree in which they are complicated with it, which it is not difficult to understand, when we find it produced by all causes of irritation, and itself the most powerful of all those causes.

It thus becomes a point of the last importance to understand its nature, that we may not only clearly perceive the principle of its treatment, but when to expect and how to obviate its first beginnings; for, evident as it is in its active, in its more chronic forms there is no disease more insidious.

In the preceding part I have considered at length the nature of those powers on which the circulation depends. The conclusions there arrived at must be kept in view, in considering the nature of inflammation.

Exp. 93. There is no difficulty, with the aid of the microscope, in perceiving the first step towards a state of inflammation. It

is well known, that exposure to the air alone, is sufficient to produce inflammation in the internal membranes of warm-blooded animals. This is also the case in the fin of some kinds of fish. The lampern was the fish I employed, and in the warm-blooded animal I employed the mesentery of the rabbit.

On bringing either of these membranes before the microscope, we see a network of vessels, too minute to be perceived by the naked eye, many capable of transmitting the globules of blood only one by one where they follow each other in rapid succession. After the part has remained exposed to the air for some time, the globules begin to move through these vessels with less rapidity, and in proportion as this happens, we perceive the diameter of the vessels enlarging, till that which could admit of only one globule now admits of several. As the motion of the globules languishes, and their number increases, their colour becomes conspicuous, which it is not while they pass in smaller number and with greater rapidity. At the same time that these changes take place, we find the number of vessels, capable of transmitting red globules, greatly increased, so that the vessels which, in the healthy state, transmitted only the colourless, are now so much distended as to admit the grosser parts of the blood. From these two causes the part assumes a redder appearance than natural, and also acquires a greater bulk; and the latter seems further increased by the distension of vessels still too small to transmit the red globules, for the interstices of the red vessels are now more opaque than before the morbid distension took place, without the appearance of extravasation of any kind.

While these changes, which may be distinctly seen with the assistance of the microscope, are going on, the part to the naked eye becomes inflamed, more opaque, and thicker.

Such then are the changes which take place in the commencement and progress of inflammation. The blood in the capillaries begins to move more slowly; these vessels in the same proportion suffering a degree of morbid distension: and this often goes

on till they, by many times, exceed the healthy diameter, and the blood in the most distended vessels ceases to move altogether.

The motion of the blood in the capillaries the reader has seen proved, by direct experiment,[1] to depend on the action of these vessels themselves. When it fails, therefore, we necessarily infer that their power is failing in the same proportion; and this inference is confirmed by their suffering themselves to be morbidly distended by the usual impulse of the blood—an effect which equally proves their loss of power. It signifies not, as I found by direct experiment, by what means the power of the capillaries is impaired,[2] whether by mechanical or chemical injury, whether by a cause operating slowly or suddenly. Any cause impairing their power produces the same effects.

During the foregoing changes, the larger vessels of the part, which are too opaque to permit the motion of the blood to be seen in them, suffer no change that can be detected by the microscope, except that, after the distension of the capillaries has become very great, the vessels immediately preceding them in the course of circulation begin to partake of the distension.

Exp. 94. Thus when the fins of the lampern were first exposed to the air, the inflammation assumed the appearance of a slight blush, in which it was difficult, with the naked eye, to discover any vessels; but, after some time, and in proportion as the smaller vessels suffered distension, vessels of a considerable size were seen by the naked eye creeping through the inflamed parts. Before this change is observed in the larger vessels, the capillaries are distended to many times their natural size, and the blood in those most distended has, generally, ceased to move. This, it is evident, cannot go very far, without the latter vessels wholly losing their vitality, and mortification ensuing.

The state of the larger vessels of an inflamed part, with the exception just mentioned, is very different from that of the capil-

[1] Part II. chapter V.

[2] The means employed were various, but the effects always the same.

laries, and may be ascertained without the aid of the microscope. The increased pulsation of the larger arteries supplying an inflamed part, sufficiently evinces their increased action: nor is there any difficulty in detecting this increased action. I have often, in inflammatory affections of the jaws, applied the finger to the external maxillary artery, both where it passes over the bone, and after it approaches the lips, and, in rheumatic affections of the head, to the temporal arteries, and perceived them beating with unusual force. On this increased action of the larger arteries of an inflamed part, the throbbing and general appearance of activity in the part depends, and on it is founded the popular opinion that inflammation consists in an increased action of all the vessels of the inflamed part—an opinion adopted without a moment's reflection on what must necessarily be the consequence of such an increased action. The reader will presently see the effect of this generally increased action, and its consequences, exhibited by a very simple experiment. The difference between what is called active and passive inflammation, depends on the degree in which the arteries supplying the blood to the debilitated vessels are excited.

We should, at first view, be inclined to ascribe the increased action of the larger arteries to the impediment opposed to the free transmission of the blood through the debilitated capillaries; but the following facts point out that it depends little, if at all, on this cause. The communications of the vessels are so numerous and free, that, as the reader will presently see determined by direct experiment, if the passage of the blood is opposed through one channel, it immediately finds another, without occasioning any apparent change in the state of the vessels concerned. The degree in which the larger vessels are excited is rather proportioned to the nervous irritation occasioned by the state of the distended capillaries, than to the degree and extent of the inflammation; for a slight internal inflammation, where the parts affected are more sensitive and sympathise more with other parts, excites the whole sanguiferous system; while external inflamma-

tion of greater severity has little of this effect; and in chronic inflammation, when the vessels have yielded slowly, and, consequently, without much nervous irritation, there is comparatively little increased excitement of the larger vessels of the part, and often even in internal parts, none at all of the whole system. From these observations it would appear, that it is to the nervous irritation occasioned by the morbid distension of the capillaries, that we are to ascribe the increased action of the larger arteries of the part. The reader has seen how immediately the action of the vessels is under the influence of the nervous system. The final cause of this increased action is evidently to support the circulation in the debilitated vessels, and excite them to a more vigorous action;[1] and the most important object of the treatment in active inflammation is to regulate it, neither to permit it to be so great as to increase the distension of the debilitated vessels, nor become so languid as no longer so to support the motion of the blood in them as to prevent stagnation and the loss of vitality consequent on it.

If the inflammation depend on a debilitated state of the capillaries alone, it follows, that whatever increases the action of these vessels, should relieve the inflammatory symptoms. This may be regarded as an *experimentum crucis* on the subject, for if exciting the capillaries of an inflamed part does not relieve the symptoms, whatever share the debility of these vessels may have in producing the disease, the co-operation of some other cause must be necessary. If, on the contrary, we find that as, on the one hand, whatever debilitates the action of the capillaries, produces inflammation, so, on the other, whatever increases their action, relieves it, nothing more is required to prove that on their debility the disease depends.

Exp. 95. I wetted the inflamed web of a frog's foot with distilled spirits, at the same time throwing upon it the concentrated rays

[1] See the Introduction to my Treatise on Sympathetic Fevers, 4th edit. pp. 24 and 25.

of the sun, from the reflector of the microscope. The blood in all the vessels, except in those of the most inflamed part, began to move with greater velocity, and, in proportion as this happened their diameters were diminished, their interstices became less opaque, and the redness of the part was lessened.

Exp. 96. This experiment was repeated on the lampern, with the same result. By gentle friction, and applying distilled spirits, the motion of the blood in the inflamed part was repeatedly accelerated, and in proportion as this happened the vessels became paler, the deeper red returning as the circulation again became more languid.

Dr. Hastings, in repeating my experiments,[1] in like manner excited the inflamed capillaries in a frog's foot, by oil of turpentine, and observed the inflammatory symptoms abate in proportion as the capillary vessels lost their increased size, and the motion of the blood was accelerated in them; and in one instance, of which he gives an account in the 90th page, this process was continued till the inflammation wholly subsided. Excessive heat and cold, in Dr. Hastings' experiments, produced languid motion of the blood, and dilatation of the capillary vessels, exactly in the same proportion as the part became inflamed. When the inflammation was caused by cold, he saw it cured by a moderate and continued application of heat, by which the motion of the blood in these vessels was accelerated, and they were made to resume their natural dimensions. When the inflammation arose from the excessive application of heat, cold produced the same effects. These facts, while they, in a striking manner, confirm the result of the experiments just related, illustrate some of the positions respecting muscular contractility, which have been laid before the reader, showing that both cold and heat, the temperature of the body being the mean, like all other agents, act upon it, either as a stimulant or a sedative, according to the degree in which they are applied. Hence appears the futility of at first view a plausi-

[1] See the Introduction to Dr. Hastings' Treatise on the Inflammation of the Mucous Membrane of the Lungs.

ble argument that cold, being the mere abstraction of a stimulant, cannot produce the effects of a stimulant, but must necessarily act as a sedative. Many other instances illustrating this observation will occur to the reader. The application of cold produces as positive a sensation in the nervous system as that of heat, and the increased contraction of the muscular fibres on its application is familiar to all. In a certain degree he has seen all agents act as stimulants, in a greater degree as sedatives, the difference between what is called a stimulant and sedative consisting, in the former, distilled spirits or heat for example, being more inclined to act as a stimulant, and the latter, tobacco and cold for example, as a sedative: but there is a quantity of tobacco, and a degree of cold, so small as to act as a stimulant, and a quantity of distilled spirits and a degree of heat, so great as to act as a direct sedative.

It is evident that the blood cannot be long retained in the debilitated capillaries, and thus, as it were, thrown out of the circulation, without some morbid changes taking place in it. Its vitality must soon cease, after its motion is wholly suspended, and the changes, to which dead blood is liable, begin to take place in it. Dr. Hastings observed, that when the debilitated capillaries were stimulated, the blood which passed from them often contained irregular flocculi, instead of globules, which he compares to the ragged portions separated from the coagulum of arterial blood.[1]

For the manner in which the various symptoms of inflammation, and means of cure, support the view of the disease, afforded by these experiments, the reader is referred to the Introduction to the fourth edition of my Treatise on Symptomatic Fevers.

Nothing can be more simple than the *modus operandi* of the means of cure in inflammation. All the local measures are such as either relieve the vessels from part of the fluid which distends them beyond their natural capacity, or more directly excite them to a more vigorous action. All the general means are such as

[1] Dr. Hastings's Treatise, p 97.

influence the impulse of the blood, either reducing it where it is so powerful as still further to distend the debilitated vessels, or increasing it, when it becomes too languid to afford the aid necessary for supporting some motion of the blood in these vessels, and thus preventing gangrene, the effect of its total failure.

It appeared to me that it would tend to throw additional light on what has been said, to subject to the test of direct experiment the principal opinions which have prevailed respecting the nature of inflammation previous to that which referred it to debility of the capillary vessels.[1] Four only deserve attention: the opinion which supposes this disease to arise from a morbid lentor of the blood clogging the minute vessels; that which ascribes it to what has been termed *error loci*, the grosser parts of the blood getting into vessels too small to transmit them; that which supposes a spasm of the extreme vessels to be its cause; and, lastly, that which refers it to a morbidly-increased action of the vessels of the inflamed part.

The reader will readily perceive that the principle of the three first doctrines is the same. In all, obstruction in some of the minute vessels is regarded as the cause of inflammation. It is surprising, therefore, that none of the supporters of these opinions thought of trying whether or not obstruction is capable of producing it. Admitting that the vessels are obstructed, it does

[1] For the origin of this opinion, see my Treatise on Symptomatic Fevers. Mr. John Allen, a gentleman whose abilities are well known to the scientific public, and Dr. Lubbock, first brought it forward in a connected form in the Medical Society of Edinburgh about the year 1790; and although imperfect traces of it, which often contradict each other, may be found of an earlier date, they appear to have been unknown to them, and are not such as can deprive them of the merit of having been the first to give a distinct and connected explanation of the phenomena of the most important disease to which we are liable. Although, as far as I know, they made no experiments for the purpose of confirming their views, their inferences were from the phenomena of the disease itself. We have just seen how amply they are confirmed by direct experiment.

not follow that an accumulation of blood will take place in the part. The blood may pass off by communicating branches, or the vessels may resist the distending force.

Exp. 97. A small hot wire was suddenly passed through the web of a frog's foot, by which the skin about the hole was shrivelled, and the vessels obstructed, no fluid of any kind being discharged. Here an obstruction was produced surely more than equal to what takes place in many inflammations of small extent, and yet no symptom of inflammation ensued, every part of the web remaining as pale as before the experiment.[1]

Exp. 98. In order to ascertain whether inflammation arises from an increased action of the vessels of the part, it is only necessary to induce such an action, and observe its effects. Having brought the web of a frog's foot before the microscope, I now and then, during some minutes, observed the velocity of the blood, which continued, as far as I could judge, the same. The foot was then wetted with distilled spirits, and, in a few seconds, the blood in all the vessels was moved with a greatly increased velocity, which, as the web was constantly kept wet with the distilled spirits, continued, as long as I observed it, ten minutes or a quarter of an hour. But during no part of the time could I perceive the slightest symptom of inflammation, either with or without the microscope. The vessels, instead of appearing redder, and more turgid, were evidently paler and smaller than before the application of the distilled spirits. The velocity of the circulation was further increased by throwing on the web the concentrated rays of the sun, from the reflector of the microscope, but still with the same effects.

In the preface to the fourth edition of my Treatise on Simple Fever, I have in a cursory way pointed out the manner in which the experiments made with a view to ascertain the nature of inflammation seem to throw some light on that of fever, which appears to be only a state of general inflammation, the whole of the

[1] Introduction to a Treatise on Symptomatic Fevers.

capillaries being debilitated, and the whole of the other parts concerned in supporting the circulation morbidly excited in the first stage; the second stage being the necessary effect of this morbid excitement when it has failed to restore activity to the debilitated capillaries, in which in slighter cases it succeeds, and the fever ceases.

In fever, the distension of the capillaries is less than in inflammation, the impulse of the blood tending to distend them, being necessarily less effectual, in proportion as they are more numerous. Hence the symptoms arising from their distension are comparatively slight; but the general fulness and redness and increased heat of the surfaces, and the general failure of the secreting power, sufficiently indicate its presence—an inference confirmed by all the plans of treatment, which have been found successful.

SECTION II.

On Congestion.

When the larger vessels of a part are debilitated and consequently distended without previous distension of the capillaries; the disease, which may be termed congestion or partial plethora, is of a nature very different from inflammation. In this case little or no distension of the capillaries takes place, as appears from the part being pale, or only slightly redder than natural. The impulse of the blood, from the debilitated state of the larger vessels, being too weak greatly to distend them, they more or less perfectly retain their power, and, as long as the larger vessels can afford any supply of blood, preserve the circulation. The reader has seen that they can support the motion of the blood, both in the warm and cold-blooded animal, long after the excitement of the larger vessels has ceased, their elastic power alone remaining. Such appears, from dissection, to be the state of the vessels of the

brain in sanguineous apoplexy, while in inflammation the larger vessels are comparatively little distended, the distension, as we have just seen, being then chiefly in the capillaries. It is an observation of writers on inflammation of the brain, that stupor supervening on delirium in this disease, is a fatal symptom: the cause of which is evident. If, while the capillaries are debilitated, the larger vessels to a considerable degree also lose their power, the circulation in the former must wholly fail.

In other parts, as well as in the brain, we constantly observe, that the distension of the capillaries is attended with acute symptoms, pain and fever, while that of the larger vessels is generally attended with little of either, being chiefly denoted by more or less tendency to a failure of function in the organ affected. The cause of this difference appears from those experiments which prove that the sanguiferous and nervous systems sympathise in their extreme parts in a way they are found to do in no other; as must necessarily arise from the capillaries, on the one hand, supplying to the nervous power the fluids on which it operates in the functions of secretion and assimilation, and, on the other, being that part of the sanguiferous system on which the extremities of the nerves by which they operate in these functions are distributed. Thus it is that the derangement attending distension of the capillaries cannot arise from that of the larger vessels; for however debilitated these vessels may be, unless the circulation in them fail altogether, in which case the death of the part soon ensues, the capillaries, as appears from statements which have been laid before the reader, are still capable of affording a certain supply of fluids to the secreting power conveyed by the nerves.

It has long been observed by physicians, that inflammation of the same organ sometimes excites acute pain and a great degree of fever, and in other cases comparatively little of these symptoms, being chiefly remarkable by the lesion of function it occasions. And this difference has been supposed to depend on the inflammation having its seat sometimes in the membranes and sometimes in the substance of the organ. Thus inflammation of

the brain has been divided into two species—phrenitis and phrenismus; namely, inflammation of the membranes and that of the substance of this organ; that of the lungs into pleurisy, inflammation of their membranes, and peripneumony of their substance, &c., and the difference of the symptoms in such cases has been supposed to depend on the nature of the parts affected. Numerous dissections, however, have now proved the fallacy of this explanation,—the substance of the organ alone having often been found affected in the most acute, and the membranes alone in the least acute cases.[1] In the former, that is the most acute cases, I be-

[1] If the reader will consult the 20th Epistle of Morgagni *De Sedibus et Causis Morborum,* particularly the 9th, 33d, 35th, 39th, 41st, 43d, 47th, 49th and 62d sections of it, and some parts of his 21st Epistle, he will find that the symptoms regarded as peculiar to pleurisy have frequently attended inflammation of the substance of the lungs, and that, when the membranes were not at all affected. When we inspect the bodies of those who die of inflammation of the lungs (says Schroeder, Opusc. Med.) they alone are sometimes found inflamed, although the symptoms of pleurisy had been well marked. Petrus Servius opened three hundred people at Rome, who died with the symptoms of pleurisy, in which the lungs were greatly inflamed, the membranes little or not at all. Tissot met with similar cases; and Diemerbroech says, that in two or three cases, in which there had been no acute pain, and where consequently, according to the common opinion, the substance of the lungs alone should have been found affected, the membranes equally partook of the disease. Burserius observes, that dissections are not wanting to prove that inflammation of the membranes has been present without any pain. Sydenham seems to go so far as to believe the substance of the lungs to be very frequently the seat of pleurisy. And Juncker, in his Conspectus Pathologiæ, observes that pleurisy often passes into peripneumony, by which we may understand that the substance of the lungs was found inflamed where the symptoms had been those of pleurisy; for such was the prepossession in favour of this division of inflammation of the chest, that when it was found that the appearances on dissection did not correspond with it, it was supposed that the one form of the disease had passed into the other—an opinion which seems to have been sanctioned even by Haller. Yet we find in some of the oldest writers more correct observations. Hippocrates speaks of pleurisy and peripneumony as affections of nearly, if not altogether, the same parts; and Galen observes that the pain in peripneumony is some-

lieve the capillaries, in the latter the larger vessels, are often the chief seat of the disease, for the two affections may, according to the nature of the cause and the state of the part affected, be combined in every proportion. I am aware that this will not always be found to be the case, for, we have just seen, the capillaries sometimes suffer distension with little or no pain, particularly where the progress of the disease is slow. In general, however, in proportion as the distension is confined to the larger vessels, there is less fever and less pain, and when they alone are affected there is little or none of either; the cause of which is evident from what has been said of the different nature of the functions of these vessels.

All local diseases producing fever seem to consist in debility of the capillary vessels of the part affected. Dr. Cullen arranges them all under three heads, Inflammation, Hemorrhagy, and serous discharge. If we examine the symptoms of the two last, we shall find, that unless these diseases are of a mere passive nature, arising from external violence or extreme relaxation, in which cases they do not excite fever, their symptoms are those of inflammation more or less relieved by discharges, in the one case, the effect of rupture of the vessels, in the other, apparently of distension of their extremities; and it is particularly to be remarked, that it is only in proportion as the symptoms of inflammation prevail, that those of fever attend. It seems then from direct experiment to be a law of the animal economy, that debility and consequent distension of the capillary vessels, and this alone of all local affections, applies to the nervous system, such irritation as excites to preternatural action the larger vessels of the part, and when of great extent or in vital parts, the whole sanguiferous system.

The reader will readily perceive how much these observations tend to strengthen the opinion of the nature of simple fever which has just been laid before him.

times acute. Many observations to the same effect might be added from authors of the first authority, both with respect to this disease and other inflammatory affections.

CHAPTER II.

On the Diseases of the Nervous System.

As the functions and sympathies of the nervous, even in the most limited and still more in the most extended sense of the term, are much more numerous and complicated than those of the circulating system, the number and variety of its diseases must in the same proportion be greater; and as it appears from the preceding investigation that its functions are not only more numerous, but, if we look only to those on which life depends, the most important, and far more so than has been supposed, its diseases must embrace both a wider and more important range than has been ascribed to them; an inference which will be amply confirmed by a review of the statements which form the subject of the present chapter. We have seen the powers of assimilation and secretion immediately depending on the nervous system, and we know that on these powers depend not only the well-being but the existence of all our organs.

If such be the facts, we cannot be more usefully employed than in an inquiry into the various causes of injury to which this system is exposed, with a view to guard against or remove them; or, where their injurious effects have taken place, to ascertain the means of correcting them, before the arrival of their necessarily fatal stage.

The present chapter is divided into three sections: the first on the diseases of the brain, the second on the diseases of the spinal marrow, and the last on the diseases of these organs jointly. Those of the brain and spinal marrow, separately, are, like the diseases of the sanguiferous system, most important, but comparatively few and simple—namely, the diseases of those functions only which exclusively belong to each. The diseases of these organs jointly are both numerous and complicated, because they

necessarily involve, we shall find, certain modifications of the diseased states of every organ of our frame. Such we should expect to be the case, from the results of the preceding investigation.

The variety and complication of diseases to which any organ is subject are necessarily proportioned to the variety of its functions, and to the extent and power of its sympathies; that is, the degree in which it partakes of and tends to produce the diseases of other organs.

The diseases of the circulating system, or of the brain or spinal marrow singly, whether original or symptomatic, necessarily consist of derangement of their own peculiar functions, and its necessary consequences; those of the brain and spinal marrow jointly, in which we have seen are lodged the immediate organs of life, directly influenced by, and influencing every organ of our frame, must suffer from, and influence, the diseases of all other parts, and thus be capable, as we shall find from direct observation to be the case, of exciting certain modifications of almost all their diseases.

SECTION I.

On the Diseases of the Brain.

We have seen, in considering certain diseases of the sanguiferous system, that when the brain is oppressed by morbid distension of its vessels, the functions peculiar to it, as might have been foretold, are impaired; a similar effect necessarily arises, if a cause existing in this organ enfeebles its powers. When the disease arises from a debilitated state of the organ itself, it has very correctly obtained the name of nervous apoplexy, the disease we are now to consider.

1. *On Nervous Apoplexy.*

The means of, in all cases, accurately distinguishing that species of apoplexy which depends on the state of the circulation in the head, from that which depends on the state of the brain itself, and consequently the proper treatment in all cases of the latter, are still among the desiderata of medicine. The object of the author in the present chapter is to ascertain how far the preceding experiments throw light on those subjects.

As it appears, as far as I am capable of judging from what has been said, that the leading features of sanguineous apoplexy depend on the fact, that the power of the heart and blood-vessels is independent of the nervous system, in consequence of which that of the brain may be overwhelmed by a compressing force without directly affecting the powers of circulation; so I think it will appear, from what I am about to say, that the leading features of nervous apoplexy depend on a fact which we have also seen ascertained by direct experiment, that the power of the heart and blood-vessels, though independent of the nervous system, may be influenced even to its total destruction through this system.

I have already had occasion to make many observations on the different species of apoplexy; I shall here, however, endeavour to present at one view whatever is essential towards a correct view of the form of apoplexy before us. I shall, in the first place, consider the consequence of such an impression made on the nervous system as greatly lessens the power of the heart and blood-vessels. If the organisation of the brain be suddenly deranged, the reader has seen, instant debility, and if the cause be powerful, a speedy destruction, of all the functions of the system ensues. In proportion as the cause is less violent, a longer period intervenes between the debility occasioned by the first impression of the offending cause, and the ceasing of the functions of life, and consequently of those of the sensorial system also; and when the cause is still slighter, provided it be of a temporary nature, the functions, instead of ceas-

ing, gradually regain their healthy state. Whatever be the result, on the first impression of a powerfully offending cause on the brain, the heart acts more frequently and feebly, and often irregularly, the circulating system suffers a similar loss of power in every part of the body, and the sensibility and other functions of the sensorial system are impaired. The sphincters of the rectum and bladder do not merely cease to be excited by any voluntary effort as in sanguineous apoplexy, but, from the impaired excitability of the muscular system, the power on which the degree of contraction constituting their state of rest depends, is more or less enfeebled.

This state is, if the offending cause has not been extreme, succeeded by some improvement in the symptoms; the heart and blood-vessels in some degree recover from the shock they received.[1] The former begins to beat with less frequency, and with more force and regularity, and the latter to convey the blood with greater velocity, and in a more uniform stream.[2] In proportion as this change takes place, the various functions, as the author has very frequently observed, improve; but if the offending cause has been severe, the heart soon begins to beat more languidly, and with it all the functions gradually and finally fail. If the injury done to the nervous system be of such a nature as particularly to debilitate the vessels of the injured part, or if the offending cause itself directly have this effect during that interval in which the vigour of the circulation is in some degree restored, the vessels of this part yielding to the increased impulse of the blood, the symptoms of inflammation are thus added to those arising from the original injury.

Such appear, from the result of the experiments detailed in the preceding Inquiry, to be the consequences of an injury of the brain or spinal marrow, capable of suddenly, and to a considerable degree, tending to derange their organisation. The reader will perceive, that if the view of the subject just taken be correct, the nervous is a much more complicated disease than the

[1] Exp. 19, 29. [2] Ibid. 91, 92.

sanguineous apoplexy. In the latter, the powers of the nervous system are impaired, but those of the sanguiferous system are, in the commencement of the disease, entire, and only become affected through the failure of the functions of respiration, secretion, and assimilation. In nervous apoplexy, not only the powers of circulation suffer directly from the injury done to the nervous system, thus producing a combination of diseased states of both systems, but the debility of the heart and blood-vessels have a secondary effect on the nervous system. The action of the brain and spinal marrow farther fail from defective circulation, and a state of these organs, analogous to what takes place in syncope, is superadded to that produced by the cause of the disease. It is not surprising, therefore, that this species of apoplexy sometimes proves instantly fatal; which sanguineous apoplexy, affecting the powers of circulation, only through the failure of other functions, cannot do, except it exists in such a degree as to produce instantaneous and total insensibility, which seldom if ever happens.

The principles of the treatment in the former case, also, are much more complicated. In sanguineous apoplexy, we have but two objects in view, to relieve the brain from pressure, and prevent its recurrence. In nervous apoplexy, while we endeavour to counteract the effects of the offending cause on the brain, it is necessary to support the circulation; the failure of which, to a certain degree, must immediately prove fatal. This ought to be done, however, in such a way as tends least to occasion morbid distension of the vessels of the head, to which the cause of the disease often renders them particularly liable;[1] tending as much through the brain, we have seen, to debilitate the vessels of every part, as the heart itself; to say nothing of the more direct effect of the injury on the vessels of the brain, which may produce either sanguineous apoplexy or inflammation of the brain, according as the distension takes place in the larger or smaller vessels. From this view of the subject we may readily understand why,

[1] Part II. Chapter ii.

although abstraction of blood often proves fatal in nervous apoplexy, yet much of the stimulant effect is often ill borne.

The simplest cases of nervous apoplexy, and those most nearly approaching to the state of the animals in the experiments just referred to,[1] are cases from mechanical injury of the brain. When a blow on the head fractures the skull, and occasions part of the bone to press on the brain without doing further injury to this organ, the case resembles in its nature the sanguineous apoplexy. When the compressing power is removed, the apoplectic symptoms disappear; but when the blow has produced what surgeons call concussion of the brain, the case is only a slighter degree of the state in which the rabbits and frogs were found after the brain had been crushed.

No writer, perhaps, has detailed the symptoms of concussion of the brain with greater correctness than Mr. Abernethy, in the third part of his Surgical and Physiological Essays. It is impossible not to remark how accurately his account of these symptoms corresponds with the results of the experiments which have been laid before the reader:—"The whole train of symptoms," he observes, "following a concussion of the brain, may, I think, be properly divided into three stages. The first is, that state of insensibility and derangement of the bodily powers which immediately succeeds the accident. While it lasts, the patient scarcely feels any injury that may be inflicted on him; his breathing is difficult, but in general without stertor, his pulse intermitting, and his extremities cold. But such a state cannot last long; it goes off gradually, and is succeeded by another, which I consider as the second stage of the concussion. In this, the pulse and respiration become better, and, though not regularly performed, are sufficient to maintain life, and to diffuse warmth over the extreme parts of the body. The feeling of the patient is now so far restored, that he is sensible if his skin is pricked, but he lies stupid and inattentive to slight external impressions. As the

[1] Exp. 19, &c.

effects of concussion diminish, he becomes capable of replying to questions put to him in a loud tone of voice, especially when they refer to his chief suffering at the time, as pain in the head, &c.; otherwise he answers incoherently, and as if his attention was occupied by something else. As long as the stupor remains, the inflammation of the brain seems to be moderate, but as the former abates, the latter seldom fails to increase; and this constitutes the third stage, which is the most important in the series of effects proceeding from concussion. These several stages vary considerably in their degree and duration, but more or less of each will be found to take place in every instance where the brain has been violently shaken."

The chief difference between the symptoms of concussion and nervous apoplexy arising from internal causes, is, that in the latter there is not so uniform a tendency to inflammation; which, in the cases referred to by Mr. Abernethy, in which we have just seen a double cause is operating, we have reason to believe causes the vessels debilitated by the blow to suffer morbid distension as soon as a certain vigour of circulation is restored. It is this renewed vigour of circulation after the immediate effect of the blow has subsided, so remarkable in the experiments just referred to, that again gives some energy to the brain, and explains Mr. Abernethy's observation, that the tendency to inflammation comes on as the stupor abates.

In nervous apoplexy, from internal causes, the sensibility is often as much impaired as in the sanguineous apoplexy. When this is the case, the danger is very urgent; but, for the same reason, as in concussion of the brain, it frequently is much less so, compared with the severity of the other symptoms and the degree of danger, because here the sanguiferous, as well as the nervous system, necessarily suffers. In sanguineous apoplexy, the derangement of function being confined to the nervous system, the danger is nearly proportioned to the degree of insensibility; but in the case before us, symptoms of the greatest danger often occur, although the patient is not wholly insensible, and not unfrequently while he

is affected with a degree of irritability. The foregoing symptoms, with the state of the pulse, afford the best means of distinguishing these species of apoplexy. In the sanguineous apoplexy, we have seen, the pulse is strong, regular, and less frequent than natural; effects produced by the diminished frequency of respiration; unless other causes of injury exist, for the two forms of apoplexy are often combined. The more general the effect of the pressure and the more free from tendency to disease in the brain itself, the more perfectly formed will the symptoms of sanguineous apoplexy be; and the contrary of these conditions indicate more or less admixture of the state we are now considering with that of apoplexy from general pressure alone: even the inequality of the pressure may tend, by irritation of the brain, to have more or less of this effect.

Such are the distinguishing symptoms of well-formed sanguineous and nervous apoplexy: and were these diseases always so formed, it would be easy to distinguish them. But we have to lament that this is by no means the case, as indeed, from what has been said, might *à priori* have been supposed. For it must often happen in apoplexy, from distension of the vessels, that the brain will sustain some further injury than that of mere uniform compression. It is not improbable that the circumstance of the compressing force, as just observed, acting partially, may sometimes alone be sufficient to produce this effect; and powerful causes, injuring the organisation of the brain, must often be of such a nature as at the same time to occasion debility, and consequently more or less distension, of its vessels. To these circumstances, and to the difficulty of distinguishing apoplexy arising from mere distension of the vessels, from that arising from an extravasation of blood or serum, it appears to me that all the difficulties respecting, distinguishing, and prognosticating the event in the different species of this disease, are to be ascribed.

It is the tendency to distension of the vessels of the brain that renders a very stimulating plan of treatment a doubtful practice,

even in the most decided cases of nervous apoplexy. Were it not for this, the state of the sanguiferous and nervous systems in these cases would equally call for such a plan. But, as might have been foreseen, the more debilitated the brain is, the more readily it feels the effects of any morbid distension of its vessels. Thus our practice in such cases is confined on all hands. Irreparable injury may be done by the free use either of stimulants or evacuants.

The mode of treatment which has appeared to me the most successful, in cases of a mixed nature, is a gently stimulating plan, combined for the purpose of preventing morbid distension, with medicine moderately determining the fluids to the surface of the body, and keeping the bowels free without occasioning a great discharge from them; with occasional abstractions of blood from the head, when the insensibility seems inclined to increase. It appears from what has been said, that the degree of this symptom is the best measure of the morbid distension of the vessels of the head, the state of the brain itself being the same.

Profuse sweating not relieving the symptoms, which is a frequent occurrence in severe cases of nervous apoplexy, seems always to indicate great danger; and to arise from a general relaxation of the extreme vessels, caused by the debilitating effect of the disease on the brain. It is analogous to the effect on the capillaries, which the reader has seen produced by crushing the brain, or the application of tobacco to this organ.

In cases arising from injuries of the head, Mr. Abernethy thinks that the great tendency to inflammation altogether forbids the stimulating plan. I have already referred to the circumstance which often makes the indication of cure in this respect different in concussion of the brain and nervous apoplexy arising from internal causes, namely, the greater tendency to inflammation in the former, arising from the local effects of the injury.

The foregoing view of the nature of the different species of apoplexy, not the result of preconceived opinions, but of facts open to the examination of every one who chooses to repeat the

experiments, and so strikingly confirmed by the observations of Mr. Abernethy and other writers on the effects of injuries of the brain, may tend, perhaps, to render the practice in this varied disease more determinate. It seems, by affording a more correct view of the nature of the symptoms of the sanguineous and nervous apoplexy, than could have been obtained without a knowledge of the relation which subsists between the sanguiferous and nervous systems, to point out, with more precision than without such aid could be done, the symptoms essential to each, and consequently the modes of practice suited to the various cases in which they separately occur, or are blended together. I have entered no further on these modes of practice than was necessary to point out the general principles on which they are founded.

Inflammation of considerable extent, or of a vital part, the reader has seen, excites increased action of the sanguiferous system. He will more readily understand here, than he would have done in the Chapter on Inflammation, why in certain inflammations the action of this system, instead of being increased, is diminished. I have just had occasion to observe, that distension of the vessels of the brain seems often, merely from the action of the distending power being partial, so to injure this organ as to give rise to more or less of the symptoms of nervous apoplexy. A similar injury of the brain, we might *à priori* suppose, must sometimes happen in that species of the distension of the vessels which produces inflammation, that is, distension confined to the capillaries of this organ; so that although in this disease the pulse is often strong, and the heat great, as in most other internal inflammations, it sometimes happens that the heat is but little increased, and the pulse small, frequent, and fluttering, more or less of the debilitating effect having been produced in the brain; the danger, for reasons just pointed out in speaking of the nature of nervous apoplexy, being very great.

A similar state of the circulation is observed in other inflammations, which occasion very great nervous irritation. Thus in in-

flammation of the stomach and bowels, the heat is often little increased, and the pulse is feeble; the brain and spinal marrow being so injured by the irritation of the inflamed state of these important organs, as to weaken the action of the heart and blood-vessels, and thus cause a greater or less degree of syncope to be combined with the original disease. I have seen the powers of circulation so enfeebled by violent inflammation of the alimentary canal, that, within twelve hours after the attack, it was impossible to obtain four ounces of blood, although large veins in both arms and both legs, and one of the temporal arteries, were opened, no blood having been taken previously, and the patient, at the time of the attack, having been strong and in good health. He died within twenty-four hours of the commencement of his disease. On inspecting the body, the whole of the alimentary canal was found inflamed, and there was a small spot on the stomach, of a purple colour, without any other morbid appearance. In all such cases, however, the pulse, though feeble, is still hard. The vessels, notwithstanding the debility induced on the sanguiferous system by the effect on the brain and spinal marrow of the irritation of the alimentary canal, still, in consequence of the obstruction caused by the debilitated state of the capillaries of the inflamed part, firmly embrace their contents. The peculiar irritation of the nervous system which attends inflammation, still excites, throughout the whole sanguiferous system, that effort to support the circulation in the debilitated vessels of the inflamed parts.

We see other causes of powerful nervous irritation producing great debility of the sanguiferous system; throwing a solution of opium or tobacco into the cavity of the abdomen, for example, immediately, that is, before it can be supposed to act through any other channel than the nerves of the part to which the offending cause is applied, enfeebles the power of the heart.

The reader may readily understand, from all that has been said, why inflammation of important organs often and sometimes very suddenly proves fatal, without the inflammation running its usual course, the derangement of the nervous system being such as to destroy the powers of circulation. This evidently happened

in the case just mentioned. He may also see why the pulse, in such cases, rises after blood-letting, which lessens the offending cause, and consequently the impression it makes on the nervous system, always a favourable symptom, both as indicating a proportional degree of vigour remaining in that system, and relieving the most urgent train of symptoms, that which both indicates the degree in which the general powers of life are debilitated, and tends still further, as we have seen, to debilitate the capillaries.

I believe that in some other cases in which the pulse rises after blood-letting, this effect, as far as it depends on the state of the nervous system, may be explained in the same way. On the same principle also, as far as I can judge, we must explain the sudden debility, and subsequent loss of power, in the circulating system, which ensues on mortification of any of the vital organs.

2. *On Suspended Animation.*

Suspended animation is the suspension of the sensorial functions from any cause interrupting respiration.

Inflating the lungs under such circumstances acts in two ways. It gives to the blood of the smaller vessels of the lungs some of the arterial properties by which they are excited to action; and acting through the blood of these vessels, it communicates to that of the larger vessels, and of the heart itself, more or less of the same properties, independently of the blood already changed being moved on towards this organ; for M. le Gallois has shown, that after the circulation has permanently ceased, the blood may be changed, by inflating the lungs, not only in the trunks of the pulmonary veins and the heart itself, but also in the great arteries. By these means the circulation in the lungs is often restored, but it is evident from the experiments which have been laid before the reader, that the function of these organs must be very imperfect till they receive the due supply of nervous power. Now this cannot happen till the re-established circulation has renewed the vigour of the brain and spinal marrow, for which a

considerable time is required. We have reason to believe, therefore, that could the due degree of this power be restored to the lungs, at the same time that they are exposed to the influence of the air, recovery might, in many cases, be effected, where inflation of the lungs alone fails.

The reader has seen that voltaic electricity can supply the place of the nervous power in the lungs, enabling them perfectly to perform their functions after the latter is withdrawn. I have therefore, in a note of some length in the chapter on the nature of death, proposed that, to the instruments used in the recovery of suffocated persons, an apparatus, properly adapted for sending the voltaic influence through the lungs in the direction of their nerves, should be added. It would be improper here to employ, for any considerable length of time, a stronger power than experience has taught us can be used without bad effects in health. The power should not, perhaps, in the present state of our knowledge, exceed that of fifteen, or at most twenty-four-by-three inch square double plates of zinc and copper, the fluid being one part of muriatic acid and twenty of water, and the electric power should not be applied for more than ten minutes or a quarter of an hour. If not too late to be of service, its good effects will be observed before the expiration of this time.

Little advantage is to be expected from voltaic electricity applied to any other secreting organ, because the revival of the patient depends little, if at all, on the action of any other. Employed as a general stimulant to the brain and spinal marrow, it may be of use by rousing the dormant powers of the system. They are all capable of being excited through these organs. In this way it can only indirectly assist the lungs, and that chiefly in proportion to the degree in which general circulation is restored. It is probable, that as a general stimulant, a greater power may be used with safety, because it may, with this view, be applied interruptedly.

[1] I mention plates of this size, because I have most frequently witnessed their effects; but I have reason to believe that a greater number of much smaller plates will better answer medical purposes.

When we compare together the whole of the foregoing statements respecting the effects of voltaic electricity, and those statements with what is said in the following section, may we not hope, that if in so few years such has been the result of its employment on the principles above laid down, a more extensive experience will still extend the advantages derived from it? I have repeatedly seen its use more successful than any other means in obstinate general debility, in which transmission through the stomach and lungs has still appeared to me the best means of applying it. In certain cases of fever, and, it is probable, in all cases of deficient nervous energy, accompanied with little or no inflammatory tendency, it will be found a powerful means of relief. The indiscriminate way in which it is at present employed must tend to prevent the advantages which a better directed employment of it is calculated to produce.

SECTION II.

On the Diseases of the Spinal Marrow.

The experiments in which different portions of the spinal marrow were destroyed, appear to throw considerable light on the nature of the diseases of this organ. The reader has seen that the destruction of any part of it not only, as is generally known, renders paralytic, that is, deprives of their only stimulus, the muscles of voluntary motion which correspond to that part, and to all parts of the spinal marrow lying below it; [1] but, by lessening the sup-

[1] It appears from what has been said, that although both the muscles corresponding to the part of the spinal marrow destroyed, and those corresponding to all parts below it, cease to move, it is from different causes; the former, because their nervous power is destroyed; the latter, because their nervous is no longer subject to the sensorial power. Whether in the former case the power of the muscles themselves is impaired, will depend on the rapidity with which the offending cause has operated. See Part ii. chap. 2.

ply of nervous power to the great chain of ganglions, influences the state of the thoracic and abdominal viscera and the temperature of the animal, consequences of which we have not hitherto been aware.

Even in early stages of diseased spine, affections of the stomach and lungs frequently attend, and the patient often complains of a sense of cold. The celebrated Mr. Pott remarks of this disease, "Loss of appetite, a hard dry cough, laborious respiration, &c., appear pretty early, and in such a manner as to demand attention." And in another place he observes, that there is "an unusual sense of coldness of the thighs, not accountable for from the weather." Similar observations are made by every writer on diseased spine. How well they correspond with the foregoing views need not be pointed out.

It appears from the experiment in which the spinal marrow was simply divided, compared with the experiments in which portions of it were destroyed, that we may judge of the extent of the injury done to this organ, in diseases of the spine, by the state of the stomach and lungs. Anything which so affects the spinal marrow as to interrupt the communication between the brain and other parts, will of course prevent the influence of the will from reaching them, however small a part of the spinal marrow may be injured. But if a considerable part is injured, along with loss of power in the limbs, the patient will experience symptoms of indigestion and oppressed breathing proportioned to the importance and extent of the part whose function is destroyed.

From what is said of Asthma and Indigestion in my treatise on the latter disease, the reader will see reason to believe, that the foregoing symptoms, namely, those indicating affections of the stomach and lungs in disease of the spinal marrow, may be relieved by the use of voltaic electricity. This observation in the foregoing editions of the present Inquiry induced the late Mr. Earle to try its effects in such cases at St. Bartholomew's Hospital. He was so good as to address to me the following letter, detailing the results:

"*George Street, August* 14, 1822.

"My dear Sir,

"I have much pleasure in transmitting to you the following account of the trials made with voltaic electricity at St. Bartholomew's Hospital. The first case is that in which you witnessed its first application.

"Elizabeth Pepperall, aged seventeen, of fair complexion and light hair, was admitted into St. Bartholomew's Hospital, in August, 1821, in consequence of an affection of the spine, which had existed for about a year and a half. At the time of her admission, it appeared that almost all the dorsal and lumbar vertebræ were affected. She had nearly lost all power over her lower extremities and pelvic viscera; and she complained of very severe cramps at the pit of the stomach, and acute pain in the course of the costal nerves, which was much increased by pressure on the ribs, or any attempt at a deep inspiration. Her general health was much deranged; her pulse was very rapid, with, occasionally, severe palpitation of the heart, and constant dyspnœa. Her digestive powers were greatly impaired; she had no appetite, and could only digest a small portion of stale bread, and some milk and water. Even this meal was always followed by uneasy sensations at her stomach, and an increase of headache, from which she was hardly ever free. Her bowels were obstinately costive, and the urine was scanty, and deposited large quantities of lithate of ammonia.

"She was placed on one of my invalid beds, which enabled her to remain in a state of uninterrupted rest; and, after the repeated application of leeches, issues were made on either side of the dorsal spine, and subsequently in the lumbar region. The issues were kept actively open, and the strictest attention was paid to her general health. The spine very gradually became less sensible, and the power over the pelvic viscera and lower extremities slowly returned; still, however, her stomach was incapable of digesting any other food than bread and milk and water, her headache remained nearly unabated, and her breathing was habi-

tually difficult. She was in this state when you saw her, and the galvanism was first administered (December 19).

"A trough containing plates of about three inches was employed. The positive wire was applied to the nape of the neck, the negative a little below the pit of the stomach. No sensation was at first produced by twenty plates; but after the sensation was excited, she could not endure more than twelve. The first sensation she experienced, caused her to take involuntarily a sudden and deep inspiration. The galvanism was applied for about a quarter of an hour, at the end of which time her breathing became much freer than it had been for many months. Of this she repeatedly expressed herself perfectly certain, at the same time she felt considerable uneasiness at the stomach. She was slightly hysterical, in consequence of the agitation she had experienced, but her breathing was tranquil during the whole evening.

"With a view to remove the tenderness in the epigastrium, leeches were applied to the region of the stomach, and the whole plan of treatment adapted to the second stage of indigestion was resorted to. When the tenderness had somewhat abated, the galvanism was repeated with more decided relief to the breathing, and without causing much uneasiness at the stomach.

"After several applications of it, the relief she experienced in her breathing lasted for two or three days, and at length it was only necessary to repeat it occasionally. The effect of its administration was uniformly the same; a most sensible and speedy relief from a state of anxious breathing to perfect ease and repose. Its beneficial effects were not, however, confined to the respiration; the powers of her stomach greatly improved, and she was able to digest a small quantity of meat, or the yolk of an egg, without pain. As her stomach improved, she lost the distressing headache, which had so constantly attended as at one time to lead me to apprehend the existence of disease in the brain, having met with other cases in which scrofulous affection had existed in the brain and spine at the same time. Her progress from this time was uniform, and far more rapid than it had been

before; and in about two months, the catamenia, which had been suspended from the commencement of the disease, returned.

" The patient was sufficiently recovered to leave the hospital, and return to her friends at Dartmouth early in July, at which time she was able to walk with very little assistance, and without experiencing the least pain in her back. On reviewing the circumstances of this case, I have not the least hesitation in stating my decided opinion of the great benefit which was derived from the employment of galvanism, not only in affording temporary relief to the breathing, but in improving the secretions, and thus materially contributing to the ultimate recovery of the patient. I feel particularly happy that the patient was in a public hospital, and that the means were employed in the presence of many intelligent medical friends and pupils, who were all equally satisfied with myself of the essential and permanent benefit which she derived from the administration of galvanism.

" It was employed in two other similar cases in the same hospital, those of Ann Baillies and Maria May, in which it produced similar good effects, except that, in one of these, the improvement of the general health, although not less than in the other cases, did not appear to have the same beneficial effect on the disease of the spine. It was tried in another case of spine disease, which was attended with fits of spasmodic asthma. These, as I was taught to expect from the observations you have published on this subject, it failed to relieve. It is remarkable that in the case of Ann Baillies, in which the pulse was from 140 to 150, and very weak, the use of galvanism always rendered it stronger, and brought it down from thirty to forty beats in the minute.

" From observing the good effects of galvanism on the secretions of the stomach, I was induced to make a trial of it in a case of deafness, accompanied with a total want of secretion of cerumen in the right ear. Its first application produced a watery secretion, which by perseverance gradually assumed the taste and

all the other characters of cerumen. The hearing was greatly improved in both ears, but how far this was to be ascribed to the restoration of the secretion is rendered doubtful, in consequence of a tumour having at the same time been removed from the tympanum of the left ear by the repeated application of caustic.

"The foregoing facts you are perfectly welcome to make any use of, should you think them deserving of notice; and I am,

"My dear Sir,

"Very sincerely yours,

"HENRY EARLE."

It appears from the preceding statement, that in disease of the spinal marrow, voltaic electricity is not only capable of performing the office of the diseased part of this organ, by which the vital functions are restored to a state of health, and the patient's sufferings greatly mitigated; but that it also, as might *à priori* be expected, by thus improving the general health, indirectly contributes to the cure of the spinal disease. In one of the cases mentioned by Mr. Earle, it failed to relieve the spinal disease, this being of such a nature, which must occasionally happen, as not to be influenced by the improvement of the general health. With regard to the last case mentioned by Mr. Earle, in which the secretion of cerumen was restored by voltaic electricity, this, it is evident from what has been said, can only happen when the fault consists in a defect of nervous influence, and not in a diseased state of the vessels.

SECTION III.

On the Diseases which have their seat equally in the Brain and Spinal Marrow.

THE present section, from the number, variety, and complicated nature of the diseases it embraces, must be regarded as the most

important division of the practical department of this volume. It is also that to which the results of the preceding investigation most extensively apply, and now for the first time makes part of this volume. At the time of the publication of the third edition, twelve years ago, (the additions made with a view to complete the Inquiry having in the interval appeared in eight papers published in the Philosophical Transactions,) I had so indistinct a view of the subject of the present section, that I judged it better not to enter on it at that time. It was in the course of the composition of the papers just referred to, with the continued opportunities of comparing the results of experiment with the phenomena of disease, that the different bearings of the subject presented themselves to me; and it is from having found that they explain practical observations which in the course of many previous years obtruded themselves on my attention, and led to a more successful treatment of a class of diseases both extensive and of very frequent occurrence, that they are here presented to the reader. Thus actual practice and physiological experiment have mutually assisted each other in arriving at the results, and, consequently, have each tended to confirm the inferences from the other; which are here stated with the more confidence, as many of my professional brethren have now, from witnessing the effects of their practical application, adopted the plans of treatment I am about to point out, and confirmed the results of my experience by their own.

It appears from the experiments detailed in the preceding part of this volume, that while the powers of the nervous system properly so called perform but a subordinate part in the functions of the sensitive system, only affording the means of conveying to the muscles of voluntary motion the dictates of the will, in those of the vital system they supply the leading power, that to which, if we except the principle of vitality itself, all its other powers are subordinate; yet it is in this system, we have seen, that the powers of the brain and spinal marrow, and of the nerves by which their influence is conveyed, have been overlooked, or seen

but in such irregular glimpses, as made no general impression on our doctrines; and left our practical inferences wholly unaided by a knowledge of the seat of the leading power in the vital system. Can we be surprised, then, that many diseases having their immediate origin in the organs of this power should in their precursory stages be obscure, an evil greatly increased, we shall find, by the insensibility of these organs, and by many other vital organs being ill supplied with nerves of sensation.

Such, it will appear from the facts I am about to state, is the frequent cause of changes, (which, when we are aware of them at an early period, may with certainty be obviated,) being allowed to proceed till they often produce effects which defy all our means.

In another respect, also, the same field in the practical department of our profession has been encumbered by a cause of a different nature.

As no serious attempt had been made to draw a correct line of distinction between the immediate effects of the sensorial and nervous powers; in like manner there had been no serious attempt to draw the line of distinction between the powers of which the animal body partakes, in common with inanimate nature, and those powers which are peculiar to itself; and it is of no small consequence in our practical inferences, that it appears from the facts which have been laid before the reader, that while the sensorial and muscular powers, and the powers of the living blood, are peculiar to the living animal; the nervous power, properly so called, the leading power in the vital functions, is in its general nature identical with one of those powers which operate in inanimate nature; it having been proved that this power does not exclusively exist in any particular organisation belonging to the living animal, and that all its functions can be effected by voltaic electricity, made to operate under the same circumstances under which it operates.

Such, indeed, has been the confusion which has prevailed on

[1] Philosophical Transactions for 1836.

this part of the subject, that the nervous influence and vital principle have been confounded and regarded as one and the same, an error from which even Hunter is not exempt; although, of so different a nature are these powers, that the latter has no existence except in the living animal, while of the former it partakes in common with inanimate nature.

The vital principle belongs exclusively to no particular set of organs, but equally animates all living parts, the organs of the sensorial and muscular powers and the blood, as well as those of the nervous power, properly so called. Is not the vital principle that which bestows on all living parts the properties which distinguish them from inanimate matter; while the nervous influence is an agent prepared only by a certain set of those parts for the preparation of which both their own peculiar properties and their endowment with that principle are equally indispensable? How confused, then, have been our views of the animal economy, when such a physiologist as Hunter could have confounded with the general vivifying principle an agent, the existence of which in the living animal depends exclusively on one particular set of its organs, and is itself of a similar nature with one of those powers of which the living animal partakes in common with inanimate nature! The vital principle might, even with more propriety, have been confounded with the muscular power, for wholly different as the properties of these powers are, the muscular power is at least one of those peculiar to the living animal.

The truth is, that the nervous influence having never been seriously made the subject of experiment, has been regarded as of so mysterious a nature, that it has been open to any fanciful properties which might be ascribed to it.

If there be any truth in the experiments publicly repeated both in London[1] and Paris,[2] and that on a great variety of animals,

[1] See the Philosphical Transactions, and the Journals of the Royal Institution, both for the year 1822.

[2] De l'Influence du Système Nerveux sur la Digestion Stomacale; par

with the same results, the nervous influence, properly so called, that is in opposition to the powers of the sensorial organs, is, of all the powers of our frame, that which is best defined, and with which we are most familiar.

The positions which are here determined by the experiments referred to, are, that besides the nerves of sensation and motion, there is a third set, the functions of which have not been understood, but which differ as essentially from the function of either of those sets of nerves, as their functions do from each other; that the functions of this class of nerves are to combine and convey an influence prepared by the brain and spinal marrow, and not by any particular part, but the whole of those organs, for the purpose of maintaining and regulating the functions of all vital organs, with the exception of those, the function of which depends simply on that of the muscular fibre—namely, the heart and blood-vessels; which organs, although deriving their power from a source wholly independent of the influence conveyed by the ganglionic nerves, are nevertheless placed under its immediate control.[1]

MM. Breschet, D.M.P., Chef de Travaux Anatomiques de la Faculté de Médecine de Paris, etc.; H. Milne-Edwards, D.M.P.; et Vavasseur, D.M.P. (Mémoire lu à la Société Philomatique, le 2d Août, 1823.) Extrait des *Archives Générales de Médecine*, Août, 1823.

[1] We have reason to believe from many facts recapitulated in the Philosophical Transactions of 1836, and republished, with some additions, in the numbers of the Medical Gazette for the 18th and 25th of March 1837, that the ganglionic nerves possess no power of bestowing sensibility on the parts to which they are supplied, and consequently that those parts derive their sensibility from the same class of nerves which bestow it on other parts; nerves belonging to this class accompanying and being bound up in the same sheath with the ganglionic, as they are found to be with the nerves of motion. At first view it appeared to me probable that in those instances where the ganglionic nerves excite the muscular fibre, they were bound up with the same class of nerves which excite the muscles of voluntary motion. But there are several circumstances which seem to point out, and others which prove, that the ganglionic nerves

Thus it is that the nervous influence, properly so called, constituting the leading power in the vital system, that power to which all its other powers are subordinate, holds under its dominion every part concerned in the vital functions, whether it derives its power from another source, or from itself; on which therefore depend the formation and well-being of all our organs;

themselves possess the power of exciting the muscular fibre. In the organs supplied by the ganglionic nerves, it is not excited in the same way as in the muscles of voluntary motion; the excitement of the muscles of involuntary motion neither being under the influence of the will nor capable of being produced, as in the case of the muscles of voluntary motion, by mechanically stimulating the nerves either of the living or newly-dead animal—a fact which we have seen misled Haller in his inference respecting the relation which the nervous system bears to the muscles of involuntary motion. But the facts which leave no room to doubt that the ganglionic nerves possess the power, under certain circumstances, of exciting the muscular fibre, are, that the muscles of involuntary motion, although they cannot be excited by stimulants applied to their nerves, either in the living or newly-dead animal, can in both be excited by stimulants applied to any part of either the brain or spinal marrow; while the muscles of voluntary motion only obey stimulants applied to the particular parts of those organs from which their nerves arise; and while the muscles of involuntary motion are more powerfully excited by chemical than mechanical stimulants applied to the brain or spinal marrow, the former of which, applied to those parts of the brain or spinal marrow from which their nerves arise, if we except electricity, with respect to which their nerves probably act merely as conductors, have little effect in exciting the muscles of voluntary motion; and lastly, we know from direct experiment, that the influence conveyed by the ganglionic nerves is the same as that conveyed by other nerves of motion, although wholly of a different nature from that conveyed by the nerves of sensation. (Philosophical Transactions for 1836.) The most ready test, as I have elsewhere pointed out, by which we may determine whether any particular function depends on the ganglionic nerves, where the parts are too minute for the labours of the anatomist, is its being subject to all parts of the brain and spinal marrow, these being the only nerves which convey the influence of all parts of those organs. It was thus that the blood-vessels were proved to be supplied by ganglionic nerves, even to their minutest ramifications. (Philosophical Transactions.)

and we cannot help observing with what care nature protects both the organs by which it is prepared, and those which convey it. The brain and spinal marrow are in all their parts defended by powerful bones; and the trunks of the ganglionic nerves, in every instance, placed so deeply in the softer parts of our frame, as to be almost as well defended as if they also had been secured by bony cases.

We are now to inquire into the nature of the morbid states of the influence conveyed by the ganglionic nerves, with a view to the improvement of the practical department of our profession; for it is impossible to conceive that a knowledge of, as far as life is concerned, the most important functions of the brain and spinal marrow; and of the only functions of the ganglionic nerves, functions of no less importance, those of combining and conveying the influence of the vital organs of the brain and spinal marrow, should not essentially influence that department; that the state of the leading power of the vital system should not be essentially concerned in its diseases.

As this is one of those powers of which the living animal partakes in common with inanimate nature, it cannot of course be subject to change by disease, but the organs which supply, and those which convey it, are as much so as other parts of our frame, and its effects must be regulated by the state of those organs.

Certain stages of their diseased states are familiar to every practical physician. But as we have neither been aware that the brain and spinal marrow supply, and the ganglionic nerves convey, the power which regulates the functions of life, our knowledge of the nature and immediate cause of many of those diseases has been extremely imperfect. From this defect of knowledge, and the want of sensibility in the parts concerned,[1] their

[1] Neither the brain nor spinal marrow appear to possess any sensibility. Previous to our being aware of the distinction between the nerves of sensation and those of motion, certain parts of them appeared to possess

early stages often excite little attention; and it not unfrequently happens that no serious attempt is made to arrest their progress, in their curable stages.

Nothing can be more evident than the inference, that if the organs of the leading power in the maintenance of the functions on which the healthy structure of every part depends, be distributed through every part of the brain and spinal marrow, those functions must be influenced by all causes which tend to impair the vigour of either, or any considerable portion of either of these organs; and no fact can be more notorious than that, in many of those instances where the nervous system has long suffered under causes of irriration, derangement of function in some organ essential to life is often at length established, which frequently resists the usual means, and terminates in a fatal derangement of structure; while in the earlier stages, from the insensibility of the vital organs, the patient's state appears to differ but little from that of others who are what is called nervous, and often continue so for a long life without any symptoms of danger supervening.

It is evident that these cases, however similar in their symptoms, must be of an essentially different nature.

The nature of the difference will be evident if we compare them with the facts above detailed, and which are recapitulated in my paper on the Powers of Life, published in the Physiological Transactions for 1836. In the one case, the derangement

sensibility, because the muscles of voluntary motion are thrown into contraction by irritating those parts. This we now know does not necessarily imply that those parts are endowed with sensibility. It is also a fact that many of the other vital organs are ill supplied with nerves of sensation—the lungs, the heart, the liver, &c. These causes have greatly contributed to the obscurity of the diseases depending on a failure or irregular supply of the influence the vital nerves convey. This may, at first view, appear to be a defect in the constitution of our bodies: but it is probably the cause of much less inconvenience than would have arisen from a high degree of sensibility in the organs the functions of which are constant, and subject to frequent and often sudden causes of excitement.

has its seat in the central organs of the sensitive system; in which, therefore, however severe the suffering, life is not endangered, because the organs of the sensitive system have no share in maintaining it: in the other, it extends to the vital organs of the brain and spinal marrow; and the circumstance of the two cases bearing so near a resemblance, arises from the sufferings being in both in the sensitive system; for the one system, for the reasons stated in the paper just referred to, never suffers without the other more or less partaking of the suffering; and all our feelings belonging to the sensitive system, the immediate sufferings are nearly the same, whether the vital organs of the brain and spinal marrow, which are devoid of feeling, partake of the disease or not; indeed, often less in the former case, as might be supposed, when the original disease is in the insensible parts of our frame; and indeed continue so, until the greater evil declares itself by the suffering of some other part of the vital system, more or less supplied with nerves of sensation, and the functions of which, consequently, being more evident, have been better understood.

The first thing which suggests that the disease may not be wholly confined to the sensitive system, is the functions of this part being more prominently and constantly affected than is common for those of any particular organ to be, in what we call nervous complaints.

Even under such circumstances, however, we are not always alarmed; we have often before seen such affections of the same part arising from causes, the effects of which proved trivial, and yielded readily to the usual remedies; and not being aware of the change which has been gradually going on in the vital organs of the brain and spinal marrow, on which the vigour of all other parts depends, we see no reason why the derangement of function should not yield as in other cases; the cough, or the headache, is a little more obstinate than usual, but we see no reason why the patient should not do well.

In such cases, however, unless we can trace the evil to its

source, and remove the cause which is preying on the vital organs of the brain and spinal marrow, we generally find that he does not do well; and are at length awakened to his real state, by symptoms of change of structure supervening on those of deranged function, when, for the most part, the disease has advanced too far to be arrested.

Of the affections of which the brain and spinal marrow equally partake, there are three forms in which the nature of the disease is essentially different. In the first, the offending cause makes its attack on the central organs of the vital system themselves. In the second, on those of the sensitive system. In the third, on some other organ of the vital system.

In order to place the nature, progress, and treatment of these diseases in a clear point of view, it is necessary to apply the results of the preceding Part of this volume, for the purpose of tracing, with more care than has been done, the laws of what in medical language has been termed the sympathy of parts, by which the complicated course of the most important of those diseases is regulated, and on which their appropriate treatment is founded; and also to inquire into the nature of those states which precede the establishment of organic disease, which, in the great majority of cases being the fatal termination, must in the course of the treatment be constantly kept in view.

Of the Nature and Phenomena of the Sympathy of Parts in the more perfect living Animal.

Although much has been written on the former of these subjects by authors of great name, the meanings attached to the word sympathy have remained indistinct, and the phenomena arranged under it ill defined; from which it may safely be inferred that we we have not been in possession of all the facts on which these phenomena depend. It is necessary, before I enter on the prac-

tical part of the present treatise, to inquire how far the various experiments detailed in the preceding parts of this Inquiry tend to throw light on them.

I shall not detain the reader by observations on what has been done by others on this subject, but immediately, by an appeal to the phenomena, endeavour to place its laws in what appears to me the only correct point of view.

It is not my intention to enter into any more extensive view of the phenomena of sympathy than is necessary to illustrate the principles on which it depends; and still less into all the various phenomena of disease dependent on it, farther than to obtain a distinct view of the nature of the diseases we are about to consider here.

It is necessary, in the first place, clearly to determine what we mean by the term sympathy, or, I should rather say, to point out the sense in which I shall employ it, for few terms have been employed with less precision. We do not refer to what is called sympathy all the effects of distant parts on each other, although there are few of these which have not, by some writer or other, been referred to it. I shall not, for example, refer to sympathy the influence on each other of the parts concerned in any act of volition, nor the effects of injury done to the trunk of a nerve on the parts in which it terminates, nor the congestion, throbbing, or other effect, from a cause seated in a part, however distant from the part affected, increasing or obstructing the circulation in it, (obstructed liver, for example, does not produce piles by sympathy,) nor, in short, any instances in which distant parts influence each other, where the structure of our bodies at once points out the channels of communication.

But when a cause, for example, which makes its impression on the stomach, produces palpitation, I shall regard it as affecting the heart by sympathy, because it at once appears from the structure of our bodies that there is no direct channel of communication between, apparently, the only parts concerned.

As it is evident, however, that no part can influence another between which there is not some more or less direct channel of communication, we may be assured that the phenomena of sympathy are produced, as in the case of all other phenomena in which distant parts affect each other by the propagation of the impression along contiguous parts, the only difference being, that in the one class the channels of communication are evident, in the other obscure; and this we shall find arises from their being more complicated as well as less readily detected.

The term sympathy, then, may be defined, the influence of distant parts on each other, between which the mere structure of our bodies, compared with the phenomena, does not point out the exact channels of communication.

Two systems, the nervous and sanguiferous, with a few unimportant exceptions, pervade every part of the body. There are no other means of communication among all its parts. As the phenomena of sympathy then, we shall find, extend to all its parts, it must be through one or both of these systems that it operates. We know that it does not operate through the sanguiferous system alone, because many of its causes are such as are incapable of directly impressing this system; and many of its effects such as no unaided affection of this system could produce. It must, therefore, be more or less through the intervention of the nervous system that its phenomena take place; but it will appear that all its phenomena are such as may take place through this system alone. We thus arrive at the conclusion, that the channels of communication here are through the nervous system, a position which has been almost universally admitted; and the phenomena of sympathy have been supposed to depend on the connexion formed by the nerves with each other, in their progress from the central parts of the system to the parts they influence.

The various parts of the living animal, as I have already had occasion to observe, may be divided into active and passive. The belly of a muscle is the active, the tendon the passive part. In

like manner, the brain and spinal marrow are the active, the nerves the passive parts of the nervous system, the latter possessing no power but that which they derive from the former. [1]

As soon as it was proved that the nerves are only the passive parts of the nervous system, it was evident that they could not be the medium on which the phenomena of sympathy depend, because these phenomena do not consist in the mere continuation of the impression from which they arise, the sympathetic effect being often of a nature wholly different from the immediate effect of that impression. The cause which excites pain alone in the part on which it operates, may excite motion alone in that sympathetically affected, and *vice versâ*. Some portion of the parts, therefore, through which the impression is communicated, must belong to the class of active parts. It must be capable, on being impressed, of originating an effect of a nature different from that of the cause which impresses it, a function of which we know the nerves to be incapable. It appears, then, that the phenomena of sympathy take place through the active parts of the nervous system, and consequently that they depend on organs which belong to the central parts of that system—a conclusion amply supported by the direct facts, and to which, by a review of these facts alone, the most correct writers of the present day have been led.

What particular connexion of nerves exists between a vital organ and the skin which covers it, between the liver and ligaments of the shoulder, between the intestines and abdominal muscles, the stomach and cartilages of the ribs? &c. Why does inflammation of the membrane of the ribs spread as readily, indeed more so, to that of the lungs, which is only in contact with it, as to the parts in continuation with it, which are supplied from the same branches both of nerves and blood-vessels? The same question may be asked respecting inflammation of the membranes of the abdomen and the head; for even the interposition of bone does not prevent this sympathy of neighbouring parts, of which the bone itself partakes. In inflammation of the bowels, we

[1] The Second Part of this Treatise.

find contiguous parts partaking of the state of each other, although their distance is great, if measured by the course either of their vessels or nerves. That the phenomena of sympathy depend on changes in the central parts of the nervous system, would appear from the fact alone, that feelings continue to be referred to a limb which is lost, at whatever part the separation has taken place. Besides, we know that all nerves convey impressions to the central parts of the nervous system, and that these parts influence all their functions—facts capable of explaining the phenomena, without any supposed action of the nerves on each other.

Here the question arises, is there a common centre of sympathy? Are the parts whose office it is to influence those secondarily affected, always the same, or are they different in different cases, so that there is more than one such centre? To answer this question, which we shall find of no small importance in the treatment of disease, it will be necessary briefly to refer to the results arrived at in the papers published in the Philosophical Transactions for 1831, 1833, and 1834, the substance of which has been incorporated with the other statements which form the Second Part of the present volume.

In these papers I have had occasion to refer to the sets of experiments, made with a view to draw the line of distinction between the sensorial and nervous functions, and determine the relation these functions bear to each other; from which it appears that the nervous bears the same relation to the sensorial, that the muscular bears to the nervous system. The power of the muscular, it appears from the facts there adduced, is independent of the nervous system, but always in some of its functions, and in all its functions occasionally, under its influence. In like manner it was found, we have seen, by an extensive set of experiments instituted for the purpose, that the power of the nervous is independent of the sensorial system, all the nervous functions remaining after the final removal of the sensorial power; but

that in some of these functions always, and in all of them occasionally, the nervous system is under the influence of that power.

Thus it appears that there are, in the more perfect animals, two systems in a great degree distinct, respiration being the only function in which the powers of both systems co-operate, that depending on the nervous and muscular powers alone, and that in which the sensorial system is included: the former constituting the vital functions, those by which we are maintained, the latter the sensitive functions, those by which we are connected with the world which surrounds us; and that both sets of functions are under the immediate influence of the active, that is, the central parts of the nervous system.

It further appears, from the facts referred to in the chapters on the Nature of Sleep and Death, that it is not with respect to their functions alone that these systems are entitled to be regarded as distinct systems. The parts of the brain and spinal marrow associated with the organs of the sensitive system, and those associated with the organs of the vital system, are distinct sets of organs, having different localities, and obeying different laws. It is quite evident, therefore, that if both the sensitive and vital functions in the various parts of our frame sympathise, it cannot be through the same parts of the brain and spinal marrow: as these functions depend on different sets of organs, their centres of sympathy must be different.

It will appear, on the other hand, from the facts I am about to state, that the phenomena of sympathy themselves lead to the same conclusion—that each system possesses its own centre of sympathy, and consequently that there is a centre of sympathy in a great degree independent of the sensitive system, and therefore of our feelings; on which, we shall find, depends one of the greatest difficulties which beset the practice of medicine, and which has led, and still leads, to errors of an extensively fatal nature, and which it is the chief object of the present section to remove.

The sympathies of the sensitive system necessarily force themselves on our attention. When the feelings of disordered digestion, for example, are accompanied by pain or sensible derangement of function in a distant part, it is impossible for us to overlook the sympathy on which such symptoms depend. But the sympathies of the vital system, operating unconsciously, are often obscure. The vital functions of both the head and chest, for example, are not unfrequently affected by such a state of the digestive organs as does not, by any complaint of the patient, call the attention to the source of the evil; and unless it be so called by other means, if the case be of a serious nature, it necessarily proves fatal; for the consequence cannot be removed while the cause continues to operate.

Another circumstance which has contributed to keep us in the dark respecting such cases, is, that the centres of sympathy in the two systems not being identical, their sympathies are not, in all instances, most prevalent in the same organs. A vital organ may be an organ of dull feeling, and little capable of influencing other parts of the sensitive system, and yet, as far as relates to the other vital organs, of the most powerful and extensive sympathy; and thus an affection which neither betrays itself to any of the senses, nor implicates organs, the sympathies of which are, from their sensibility, the most prominent, may be undermining all the powers of life; and I think all conversant with the practice of medicine will admit that it is here that it is at present most defective. Fatal cases are every day occurring, as appears from dissection after death, the progress of which might have been easily checked, had we been aware of their nature before the secondary and more prominent affection had shown itself; and even after it had appeared and made some progress, had we been aware of the cause which was supporting and aggravating it; for few affections are, from the first, necessarily of a fatal nature.

There is no organ whose sympathies are absolutely confined either to the sensitive or vital system,—all organs, more or less, partaking of the functions of both: but that the different species

of sympathy prevail most in different organs, a thousand phenomena assure us; and we have ample proof that the vital often so little influence the sensitive sympathies, as neither to attract the attention of the patient nor his medical attendant.

In no other organ are the sympathies of the sensitive system so powerful as in the stomach—an organ of the most acute sensibility; but the sympathies of the vital system are much more powerful in the liver, which, although of very dull feeling, influences, and is influenced by, the vital functions of distant parts more powerfully (if we except the brain itself) than any other organ; and that, it will appear, from what I am about to say, in a degree that admits of no comparison.

We are now to premise such observations on the process by which organic disease is established, as are necessary for a clear understanding of the nature and treatment of the diseases we are about to consider.

On the Process by which Organic Disease is established.

It will not be difficult, I think, with the aid of the experiments detailed in the preceding part of this Inquiry, relating to the functions both of the nervous and sanguiferous systems, compared with the well-known laws of the animal economy, to ascertain, up to the moment at which change of structure begins to take place, when, as in the great majority of cases, it is the effect of evident derangement of function, the process by which organic disease is established.

When we attempt to advance farther, our difficulties are greatly increased; and were we capable of ascertaining the various changes which constitute the different forms of organic disease, we have so few means of influencing them, that it is probable their treatment would be little improved by this knowledge.

In most instances, on the other hand, we possess means which

powerfully influence the states which precede it, and the better these states are understood, we shall be the better enabled to perceive the first tendency to organic change, and regulate the means which tend to prevent it, and thus to prevent diseases which it is so little in our power to remove, or even greatly to alleviate.

On the powers of the sensorial, nervous, and muscular systems, and the powers of the living blood, and the relations these powers bear to each other, all the functions of life, more or less, immediately depend, and consequently all rational systems of treatment must be founded.

The knowledge of particular functions is necessarily of slow growth. It must be the result of many minute and laborious investigations; and although much has been done in this department by able physiologists, it must still be regarded as in its infancy. But however carefully individual functions may be studied, it is evidently impossible that they can be understood without a knowledge of the general laws to which they are all subjected. This, therefore, is the first object which demands our attention.

Thus it was that, after the revival of science, the attention of physiologists was in the first instance directed to determine the source and nature of the nervous and muscular powers, and the way in which they influence each other in their various functions.

In the preceding parts of this volume we have seen the difficulties which lay in our way, and it will appear, I think, from what I am about to say, that we are now prepared for the task here proposed.

We know from ample experience that all derangement of function tends to derangement of structure; the time required for this effect being different according to the state of the particular constitution, the nature of the part affected, and the nature and degree of the derangement produced in it.

The most frequent causes of derangement of function make

their impression on the nerves of the part, and their effects may be divided into two stages. The first is merely a state of nervous irritation from causes acting on the part itself, or some part with which it sympathises. In neither instance, in this stage, is there any disease in the part to which we refer it. To whatever part the cause of irritation is applied, the immediate cause of suffering is in the central parts of the sensitive system, and is only referred to the part to which we refer it, in consequence of experience having associated certain feelings with certain parts of our frame.

The suffering of the sensitive system does not long continue without the central parts of the vital system, properly so called, by the sympathy which exists between it and the neighbouring parts, partaking of it. Thus the nervous power requisite for the functions of the part impressed by the offending cause at length fails; and, in consequence of this failure, its extreme nerves, on the co-operation of which with its extreme vessels, or rather with the fluids they convey, all its assimilating functions immediately depend, begin to be incapable of their part in these functions. Thus disease of function in the part itself is induced, but this also arises from the state of the central organs, and there is still no farther disease of the part itself than arises from the irritations of the vitiated secretions.

To a certain point, the vessels accommodate themselves to the change; for the resources against the establishment of disease in every part of the system are powerful. The functions of the part are more or less disordered, for one of the powers on which they depend is more or less enfeebled; but the vessels still maintain the healthy diameter and a free motion of the blood, and for some time there is no evidence of the debilitated state of the nerves having spread to them.

The two stages I have now described are thus established; the only difference between them being, that to the mere nervous irritation constituting the first, the vitiated secretions of the part originally impressed by the offending cause, in consequence of the continuance of this stage, are now added. These may be re-

garded as the two first stages tending to organic disease. If the first is not removed, the second, in most cases, will soon supervene, and this stage seldom lasts long without bringing the part into the state which immediately precedes that disease, unless the nature of the part affected obviates this consequence.[1] The usual effect of the continuance of that state of the nerves of the part by which the function suffers is, that the debility at length extends to the capillary vessels which supply the fluids on which the influence conveyed by the nerves operates in the functions of assimilation and secretion. Thus a state of inflammation, either acute or chronic, is established. Under such circumstances the texture of the blood suffers, and the next step affects the structure of the part, sooner or latter, according to the nature of the part affected, and according as the inflammatory state is more or less acute.

Such is the succession of events when the offending cause tends directly to debilitate the nerves alone. It may, however, act directly on the vessels alone, which only happens in cases of rare occurrence, or on both, the last being the process most frequent in acute, the first in chronic disease; and here, as in all other instances, whether the offending cause acts on the nerves or vessels, or both, instead of directly debilitating, it may in the first instance act as a stimulant, and the first effect on both be that of increased excitement, the debility being only consequent on this effect.

The whole of both processes, as far as relates to the vessels, that is, whether the offending cause acts as a stimulant or direct sedative, may be distinctly seen in the transparent parts of living animals, as I have often witnessed, with the aid of a microscope of moderate power.[2] While the offending cause acts as a stimu-

[1] As is often the case with respect to the liver in this climate, the function of which will sometimes continue deranged, and even after the next step has in some degree taken place, for a long lifetime, without its structure becoming affected; but I believe there is no other organ to which this observation applies.

[2] Introduction to the Second Part of my *Treatise on Febrile Diseases*. 4th edition.

lant, its effects, as might be foreseen, are found to be those of lessening the capacity of the capillary vessels, and in the same proportion increasing the velocity of the blood in them; the inflammatory state only supervening in proportion as the vessels, exhausted by the increased action, begin to lose their power.

In the one or other of these ways, a state of debility, both of the extreme nerves and capillary vessels of the part, is always at length induced by continued causes of irritation.[1] If the cause be such as equally affects the nerves and vessels, the power of both fails together; if chiefly the nerves, their power is impaired by the process just described, and their debility never fails at length to be communicated to the vessels with which they are associated in all their functions, and which are, equally with the nerves themselves, although in a different way, under the influence of the brain and spinal marrow.[2]

Thus inflammation of the part, of an acute or chronic nature, according to circumstances, is established; and it is evident from what has been said, that the tendency to disease of structure, *cæteris paribus*, must always be proportioned to the degree in which this takes place; that is, in proportion to the derangement of those organs, namely, the extreme nerves and vessels, on the healthy co-operation of which the due structure of every part depends. Hence in chronic cases the degree of tightness of pulse is always found one of the best measures of the tendency to organic disease; and hence, in its prevention, the great importance of anti-inflammatory measures, as far as the state of the strength will admit of them: and the great injury done by every cause which tends to increase the inflammatory tendency beyond what is essential to the maintenance of the general strength; for the greater the debility, the more intractable all diseases become. The art of medicine is directed to second the efforts of nature; and in proportion as her efforts fail, ours necessarily become ineffectual.

[1] My papers in the Philosophical Transactions for 1815, 1829, and 1833.

[2] See the Section on Inflammation in a following part of this volume.

The way in which the inflammatory state operates in effecting the various changes which constitute the different species of organic disease, as I have already had occasion to observe, must be the subject of future investigation. In the mean time, the information of most consequence is the nature of the states which cause change of structure, and the best means of counteracting them; because, after it has taken place, if we except certain organic affections of the liver, and a few other parts over which the great powers of mercury give us some control, it is seldom in our power essentially to influence its progress.

There is one instance, however, in which the structure of the part suffers, and in which the change is simple, because it consists in the mere destruction of the healthy organisation, not in the establishment of any new organisation of the parts affected, and is evidently but the continuance and consequent increase of the change which has been going on from the commencement of the disease.

We have seen that all the changes which precede organic disease indicate loss of power in the vital functions of the part. From an early stage, either its vessels or nerves, or both, are debilitated. If this failure of power proceeds without the interference of any cause to disturb its course, it is evident that it must terminate simply in a total loss of those functions. The part must wholly lose its vitality, and become subject to the laws of inanimate matter. Such is the termination in gangrene; a change, for evident reasons, which more peculiarly belongs to active, as new organisation, if we except suppuration, to chronic inflammation; because the latter change generally requires a considerable time for its accomplishment.

1.—*On a debilitated state of the Vital Organs of the Brain and Spinal Marrow, when the offending cause is seated in these Organs themselves.*

As we have seen that on an agent supplied by the brain and

spinal marrow the functions of assimilation and secretion depend, it necessarily follows that the derangements to which the immediate organs of these functions are subject may be of two kinds. From whatever other causes they may arise, they may either be the effect of causes acting directly on the latter organs themselves, or on those organs which supply an agent essential to their functions; and this inference, from all that has been said of the assimilating and secreting functions, we shall find amply confirmed by the course and consequences of their derangements.

As all discussions are the clearer the more definite they can be made, it will be the most distinct plan to consider, in the first place, the derangement of one particular organ, or set of organs; and when the principles are illustrated by the phenomena which attend and are consequent on them, their application to all other cases of the same kind will be easy; and I shall make choice of the digestive organs, both as those assimilating organs of the most powerful and extensive sympathies, and those the functions of which are most easily made the subject of observation.

In conformity with the results of the experiments above referred to, we find that all diseases affecting any considerable portion either of the brain or spinal marrow, more or less derange the assimilating functions; and from the greater sensibility and more evident functions of the digestive organs the effect is generally first, and to the greatest degree, perceived in them. Even a piece of bad news will instantaneously, either by its direct effect on the nerves of the stomach, or by producing a vitiated secretion of gastric juice, destroy the appetite; and mental causes, of a serious and permanent nature, sensibly derange the assimilating functions in every part of the frame. We find similar effects from diseases or accidents affecting any considerable portion either of the brain or spinal marrow. These consequences are as certain as that a vitiated secretion is the consequence of disease of a secreting organ. When such facts are considered, it seems surprising that, independently of all experimental research, it had not occurred to physicians, that in cases of chronic derangement of the assimilating

functions, as in more acute affections, the fault might sometimes be in those organs. But being prepossessed with the opinion that they were organs of the sensitive functions alone, it was only in the more striking cases that the truth was forced on their attention.

Another circumstance has greatly contributed to the same effect. It appears from what has been said that the centres of sympathy in the vital and sensitive systems are not identical; the functions of these systems, although wholly in the sensitive, and chiefly in the vital system, depending on organs which belong to the brain and spinal marrow, not depending on the same organs. Hence we have seen it is, that there is a centre of sympathy independent of the feelings, many of the vital organs being parts of dull sensation; from which, we shall find, the most important practical errors have originated.

From the nature of the investigations in which I have been engaged, and the importance of the digestive organs in the animal economy, my attention was at an early period directed to them, and particularly attracted by finding that cases of indigestion occasionally presented themselves, which, although on the whole similar to the usual forms of the disease, ran a very different course,—at first not differing in any remarkable degree from the more usual cases, but at length assuming a formidable shape, without any distant organ being implicated, which is almost always, in this country, the precursor of danger in ordinary cases of indigestion, and without any more formidable disease of the digestive organs themselves having made its appearance. Death seemed to arise from the failure of the digestive process alone; there was no prominent symptom that was not referable to its organs; and the patient, emaciated to the last degree, appeared to die of inanition, in consequence of these organs, even where food could still be taken, being incapable of effecting the necessary changes on it.

It was in considering these cases, and comparing them with the effects I had witnessed from preventing a considerable part of the

influence either of the brain or spinal marrow from reaching the digestive organs, that I was led to suspect that the fault might be in the central parts of the nervous system; and on examining the bodies of those who died in this way, I found the brain diseased, and particularly in the parts towards its base, from which the vital nerves proceed.

These cases had often, in their more early stages, been treated as cases of simple indigestion, and the friends assured, that although, being more obstinate than usual, they would be tedious, there was no danger to be apprehended from them; and I have seen some of the most eminent of our profession surprised when I expressed an opposite opinion, in which, from the course of the disease, they themselves were at length obliged to join me.

I need not say that it is of essential consequence to be able to distinguish these cases from those of ordinary indigestion at an early period—the only period at which there is any hope of arresting their fatal course.

I shall, in the first place, point out the best diagnosis at which I have been able to arrive; for it will readily be perceived, by those acquainted with the principles of our profession, from what has been said, that there must be great difficulty in such a diagnosis. I shall then give an account of the appearances on dissection, referring to those in other cases of a similar nature, but of more general derangement, for the purpose of illustration; and, lastly, concisely point out the general principle of treatment, from which there is no hope, except in the earlier periods of the disease.

In the first place, of the diagnosis of the case before us. I have just had occasion to observe that the organs of assimilation must not only be exposed to disease from causes operating on these organs themselves, but on those organs also of the brain and spinal marrow, on the agent supplied by which their functions immediately depend; but as in both instances the disease consists merely of symptoms indicating derangement of the organs in question,—the digestive organs for example,—and a certain train

of nervous symptoms, in the former case arising from their derangement, the intelligent physician at once perceives the difficulty of distinguishing them; the patient either never complaining at all of the head, or only of such affections of it as we are constantly meeting with, as consequences of common indigestion. Yet it is evident that these cases must require very different plans of treatment, because, in the one, if we restore the digestive organs, the nervous symptoms, the mere consequence of their derangement, necessarily disappear; but in the other there are no means of restoring the digestive organs themselves, unless we can correct the disease of the brain or spinal marrow, or perhaps both, on which their derangement depends: for it follows from the experiments above referred to,—and we shall find the inferences from them amply confirmed by the phenomena of disease, as well as by the treatment which relieves them,—that the affection of either or both may cause the symptoms we observe.

The difficulty is greatest, however, when the cause is confined to the brain, because, as we have seen, the affections of the spinal marrow are generally attended with such local symptoms as necessarily call the attention to the seat of the disease. It is, therefore, to the diagnosis of the former cases that I shall here direct attention.

The nature of the cases in which the original cause of the disease is confined to the brain, precludes the possibility of deriving the diagnosis from any particular train of symptoms: it must be collected from a review of the whole circumstances of the case; from the nature of the remote causes, both predisposing and occasional, the general course of the symptoms, and the effects of the means employed. I shall enumerate the circumstances which chiefly demand attention, and endeavour more particularly to point out the principles on which the diagnosis must be founded.

When the patient is not of a variable and hysterical habit,—when the occasional causes have been of a serious and permanent

nature, and particularly such as directly act on the brain, and the nervous symptoms have not shown themselves for some time after the first application of such causes,—when there is not such derangement in the digestive or other organs chiefly affected as accounts for the severity of the nervous symptoms,—when the affections, both of mind and body, are less variable than is usual in what are called nervous complaints, and particularly when they are apt to be referred to the same parts of the body,—when there is constantly a more or less general tendency to derangement in the secreting system,—when the heart is more irritable and the lungs less free, the nervous symptoms not yielding so readily as usual, the depression of spirits more uniform, and the pulse tighter than we should expect to find it from the other symptoms,—when either the recurrence of feverishness or a sense of chillness and debility is more frequent than is usual in nervous complaints,—when the constitution seems more affected than usual by the continuance of the disease, the strength on the whole decaying,—and particularly when the countenance assumes a sallow colour and an habitually irritable and anxious expression; when the usual means of cure are not attended with their usual effects, our stomachic medicines being in a great degree powerless, and alteratives producing but a transitory, if any, improvement in the abdominal secretions; when these, or several of these circumstances, are well marked in what are called nervous complaints, I have been assured, by repeated observation, that they are not to be safely disregarded.

The diagnosis is much assisted by observing the nature of the nervous symptoms in the two cases. There is in our frame, we have seen, what may in a great degree be regarded as two distinct nervous systems—the sensorial and vital. The sensorial functions may be disordered for a great length of time without endangering life, the vital functions, with the exception of respiration, having no dependence on them, and respiration not being endangered till their derangement is extreme; but disorder of the vital system cannot go far without danger; and from our mis-

taken views of the functions of the nervous system it often happens, both where the disease has originated in its vital parts, and where it has spread from the sensitive to the vital parts, that danger is frequently unsuspected till, in consequence of the failure of nervous influence, disease is established in some vital organ.

Thus it is that, in all cases of nervous debility, it is necessary to examine with care the nature of the functions chiefly affected. If these be the mental functions, and we find that there is little or no affection of vital organs but such as is evidently the effect of their derangement, whatever be the sufferings of the patient, (and these, from the chief derangement being in the organs of the sensitive system, are often greater than where there is more danger,) we may be assured that life is little, if at all, threatened. If, on the contrary, the organs of life chiefly suffer, and that independently of mental affections, (especially if the course of the disease be more uniform than that of nervous affections usually is,) however purely of a nervous nature the symptoms may be, and however little formidable either in appearance to others or to the feelings of the patient, danger is to be apprehended, and, if the pulse be decidedly tight, is not far distant. I have, in my *Treatise on the Preservation of Health, and particularly the Prevention of Organic Diseases*, entered at length into the nature, diagnosis, and treatment of such cases; the fatal termination of which I have often witnessed. Having been confounded with the less important nervous affections, their fatal tendency has frequently been so much overlooked, that when it at length showed itself, either by a decided affection of some vital organ or unequivocal symptoms of fatal inanition, it has sometimes found the physician, as well as the patient, unprepared.

By a due attention to the whole of the foregoing circumstances, we may generally distinguish the disease before it is far advanced; and I have reason to believe, from many cases which have come under my care, often succeed in arresting its progress

by the means I am about to point out. In the mean time the nature of the disease will be further illustrated by turning the attention to the appearances on dissection after death.

This part of the subject will be best illustrated by giving the appearances on dissection in two cases, which, in their early stages, had been treated as common nervous and bilious complaints; in which I had stated to the other medical attendants, that, notwithstanding there were no symptoms referred to the head, we should find the brain organically diseased.

The first case I shall mention is that of Mr. A., who was taken ill while pursuing his studies at Oxford. His case was regarded by the physicians of that city as one of common indigestion. His health not improving, he was brought to London, and placed under the care of two physicians well known to the profession here. After he had been in London a few weeks, I was called in, in consultation, and, guided by the foregoing circumstances, expressed my fears of a fatal termination, and stated my opinion, in consultation, that although the stomach and duodenum were the organs most prominently affected, I believed we should find the origin of the disease in the brain; and on dissection after death, which happened in a fortnight or three weeks after I saw the patient, and appeared to be the consequence of inanition, the following appearances presented themselves.

The body was examined by Mr. Walker, of St. George's Hospital. In this and the following dissection the examination was made about twenty-four hours after death, and the body was free from fetor. The following is his report: —

"On opening the cavity of the cranium, the membranes and the brain were found tolerably healthy, perhaps rather softer than usual, particularly as regards the cerebellum and base of the brain, which, together with the medulla oblongata and cerebral nerves, appeared reduced to a pulpy state; so much so, that they would not bear the slightest handling.

"The viscera in the cavity of the chest presented no unusual

appearances; the stomach larger than usual from distension, and presenting that appearance which is called the 'hour-glass contraction' of that viscus in a more marked manner than is usually met with; the pylorus much more vascular than usual, and the duodenum much more dilated, vascular, and attenuated, than is natural. The whole of the small intestines were more distended with flatus, and much more gorged with blood, than in the healthy state, and of a very dark colour. The liver, spleen, kidneys, and pancreas, were healthy."

The following case was that of Miss C., which ran the same course as the preceding, but was of longer duration, having been protracted for more than two years; and here also the patient appeared to die of inanition. Some surprise was expressed that I should wish the head to be examined, as none of the symptoms had been referred to it. The examination was made by Mr. Earle, and the appearances in the brain corresponded, in a remarkable degree, with those just detailed. The symptoms in these cases, as well as the termination of the disease, had been similar, and we find the chief organic affection of the brain of the same kind, and seated in the same parts. The following is Mr. Earle's account of the appearances:—

"In the head, slight effusions beneath the arachnoid membrane; substance of the brain very soft, particularly the crura cerebri and upper part of the pons varolii, which was quite pulpy. Blood-vessels in the substance of the brain large, and loaded with blood. In the chest, right lung greatly compressed by the narrowness of the inferior margin of the ribs, from old adhesions between the pleura costalis and pulmonalis. Substance of the lungs firm and hepatised. Left lung more healthy than the right, but slightly hepatised at its upper part." This state of the lungs, it may be remarked, is peculiarly characteristic of a failure of nervous influence, as appears from those experiments in which the influence of the brain was prevented from reaching the lungs. The patient had been subject to cough and oppressed breathing; pulmonary symptoms, however, had never been a

prominent part of the disease. "The heart," Mr. Earle proceeds, "was remarkably small. In the pericardium, about two ounces of water. In the abdomen, stomach and duodenum much displaced by the compression of the chest by the stays. Towards the pylorus, the stomach much thickened and indurated, the pylorus hard and contracted. The duodenum large and flaccid; the mucous surface very vascular, villous, and soft, readily breaking down on the slightest touch, and apparently approaching to a state of ulceration. Liver almost of a black colour, and gorged with venous blood: substance of the liver hardened. Spleen and kidneys small, but not unhealthy. Intestines generally of a dark colour, from venous congestion."

The circumstance of more general organic disease being found in this than in the preceding case, I shall presently have occasion to explain.

Cases like the foregoing, in which the patient wastes without an apparent cause capable of accounting for the degree of wasting, (for he sometimes takes a considerable portion of food,) have been often ascribed to mesenteric obstruction, which dissection has disproved, but without throwing light on their real nature, because the necessity of examining the head has not occurred, none of the leading symptoms having been referred to it; and had it been examined, the appearances observed could not have been connected with the course of the disease, while the brain was regarded as the organ of the sensitive functions alone.

Such cases are not the consequence of the chyle being prevented from entering the blood, but of its not being formed, the processes by which it is formed having been suspended by the failure of nervous influence; for we have seen that the influence even of any considerable part either of the brain or spinal marrow being withdrawn, is sufficient to derange the process of digestion.

There is a case belonging to the same class (although no cases can differ more in their symptoms than it does from the preced-

ing cases) to which I have already referred; the consideration of which is necessary to a clear understanding of the nature of that class of diseases. When the powers of the different organs are so well balanced that no part becomes the seat of a very prominent affection, and thus, as it were, draws to itself the effects of the failure of nervous influence acting on the principle of an issue, but much more powerfully with respect to other parts, and at length, by proving fatal, cutting short the disease before that of the brain has had time to run its course;—I say, where no part thus becomes the most prominent seat of the disease, the case necessarily assumes a very different form, and, if it be allowed to proceed, terminates by loss of power in the brain itself—a case of very rare occurrence; for the usual termination of such cases, we shall find, is some particular vital organ being disorganised from a failure of nervous influence, before the state of the brain can become such as to prove fatal.

We may infer from what has been said, that we should find, on examination after death in such cases, a general tendency to disease of the vital organs, the disease having run on till the want of nervous influence was felt throughout the system; and more or less general derangement of structure throughout the system had consequently taken place, but none to such a degree as itself to prove fatal. It will best illustrate these observations to lay before the reader an account of a case of this kind, with the appearances on dissection after death.

Mrs. W., a lady between forty and fifty, had from time to time been under my care for some years. She had, more or less, laboured under indigestion, with occasional symptoms of derangement, sometimes referred to one part, sometimes to another, which were from time to time relieved; and on the whole, although debilitated and what is called nervous, she was for the most part capable of the ordinary duties of life. By degrees the symptoms referred to the head became a more prominent part of the disease. She had been absent from home for some months, during which the affection of the head had

greatly increased, and returned in such a state that she soon became apoplectic, and only survived her return about a fortnight.

The body was examined by Mr. Jefferson of Islington, and the following is his account of the appearances observed:—

"The skull was remarkably thin; in most places not thicker than a shilling. The coverings of the brain very turgid with blood, (you would rarely see them more so in a complete case of apoplexy,) with a deposition of serum and coagulable lymph between the arachnoid and pia mater. The substance of the brain itself was very firm, and much more vascular than natural; there was rather more water in the ventricles than usual, but no great quantity.

"The lungs were very unhealthy on both sides, being studded with small tubercles, many in a state of suppuration, and others approaching to it. In the heart there was nothing remarkable; perhaps rather paler than natural.

"The liver remarkably firm in texture, and rather paler than natural, but no very morbid appearance in it; the gall-bladder rather larger than natural, and distended with thick viscid bile, and containing fourteen gall-stones, bigger considerably than as many large peas. There did not appear to be any of them in any of the ducts. The stomach was rather smaller than natural, the coats of which were much thickened; the internal, or villous, so firm that it could not be easily torn. The pyloric extremity showed more vascularity, as if from the effect of recent inflammatory action; and it adhered for a considerable extent to the diaphragm and left lobe of the liver. There was nothing particular throughout the remainder of the alimentary canal. The spleen larger than natural: the bladder much distended, but no disease; the uterus remarkably firm, so as to give a cartilaginous feel upon cutting into it; the os uteri very vascular, with a small polypus excrescence from the neck."

We here see a striking instance of the effects of long-continued defective nervous influence. The lungs were very unhealthy, and studded with tubercles, although the disease had never ap-

peared in them in an active state. The secreting power of the liver had been greatly deranged, and this organ was found diseased in structure. The same was true of the stomach, spleen, and uterus. The brain itself, also, was organically diseased, and the patient, none of the secondary affections proving sufficient to destroy life, died in consequence of such morbid distension of its vessels as caused a fatal compression.

The difference in the course of the disease in this and the preceding cases may have been influenced by the affection of the brain being of a different nature.

We see the same tendency to general organic disease in the second of the above-mentioned cases, which, like Mrs. W.'s, had been of long standing, but in which the disease of the brain was cut short by a total failure of power in the digestive organs. In the first case the organic disease was chiefly confined to the duodenum, its state being such as to prove fatal before the failure of nervous influence had had time to produce much effect in other organs less disposed to disease; this case having only lasted a few months, and the tendency to disease in the digestive organs, arising from peculiarity of constitution or other causes, tending to protect other parts.

I need hardly say, that it appears from the facts detailed in the preceding Part of this volume, that when the cause originates in the brain, the tendency to derangement of function must be general, including the functions both of the sensitive and vital system; and such we find is the case. My limits do not permit my entering on the detail of treatment, for which I must refer to my Treatise on the Preservation of Health, and particularly the prevention of organic disease. The great object in the prevention of functional degenerating into organic disease is to restore and maintain the healthy functions of the part affected. In the case before us, therefore, the treatment, which can only be successful at a very early period, is to correcct the deranged and maintain the healthy function of whatever organ, whether of the sensitive

or vital system, in which the derangement shows itself; and consequently the patient's only chance of recovery depends on the nature of his disease being ascertained at an early period: for it is only before any degree of disorganisation of the brain supervenes, that a treatment founded on this principle can be successful; and in such a case, at least according to my experience, no other will avail.

Such are the forms assumed by a debilitated state of the vital organs of the brain and spinal marrow, when the offending cause immediately influences these organs themselves. This, which, of all cases belonging to the present section, is the most obscure in its early stages, and in all its stages the most difficult of treatment; is fortunately the most rare.

It will appear from a very cursory review of the results of the preceding Part of this volume, that it is only in certain rare cases, where no particular vital organ is more inclined than the rest to organic disease, or where the disease is a partial affection of the vital organs of the brain and spinal marrow, and originates in some parts of those organs themselves, that change of structure can take place in them; because it was found, on the one hand, that any considerable diminution of their general power is sufficient so to derange the structure of some other vital organ as in this way for the most part to prove fatal before any general affection of these organs could be such as to terminate in change of structure in them; and on the other, that when the disease originates elsewhere, its operation on them must always be a general, not a partial operation, because we know from direct experiment that all other parts receive nerves from all parts, and consequently must be capable of influencing all parts of them.

2. *On the Diseases of the Central Organs of the Vital System, in which the offending cause makes its impression on those of the Sensitive System.*

With respect to that form of the disease which originates in

the central organs of the sensitive, from which it spreads to those of the vital system, its diagnostic symptoms and general course are the same as in the forms of disease we are next to consider, except that they are preceded by suffering of the sensitive system, and attended by greater and, compared with the other symptoms, more prominent derangement of that system, than in them; for such is the sympathy which exists between the central organs of the two systems, that in all cases they each partake of the affections of the other, of which their vicinity alone, we have seen, is a sufficient cause.

In the case before us, we have reason to believe from all that has just been said, that when the functional derangement of the central organs of the sensitive system—that, for example, caused by a settled grief—debilitates by sympathy those of the vital system, it will, long before it can produce organic disease in the latter, so derange the supply of nervous influence to the vital organs throughout the system, as to cause a fatal disorganisation in some of them. It is possible, although, from the vicinity of the central organs of the two systems, it probably rarely occurs, that the derangement of those of the sensitive system may terminate in their disorganisation, before the central organs of the vital system have so far partaken of the derangement, as to produce a fatal disease of structure in a distant part, the usual termination where such a case proves fatal.

As the principles which operate, both in the production and treatment of such cases, are the same as in those of the diseases which form the subject of the next division; and as the offending cause operating in the sensitive system is always an evident one, without the removal of which no plan of treatment can be successful; and with that, in all curable stages of the disease, little else is required to secure recovery; there is little to detain us in the present division of the subject. I shall have occasion in that which follows, which is on every account the most important, again to refer to the diseases which belong to the present division.

3. *On the Diseases of the Central Organs of the Vital System, in which the offending cause makes its impression on a distant part of that System.*

The diseases arranged under this division are important on every account which can render a disease worthy of careful consideration. We shall find them among the most frequent and complicated of all diseases; in their early stages, although almost uniformly curable, attracting little attention; in their advanced stages, both difficult of treatment and of doubtful prognosis; and in their last stages, with few exceptions, necessarily fatal.

The circumstances which chiefly characterise the diseases before us, are their little apparent consequence in the commencement, and the little apparent connexion which their first stage has with their succeeding course. From these circumstances the danger in a great degree arises; and the obscurity is not a little increased by the disease, in many instances where the nervous system is naturally strong, and encounters no other cause combining with the disease to impair its powers; never advancing beyond its first stage, which tends still farther to confirm the opinion of its innocence, by tending to prevent our associating it with its more formidable stages.

I shall, in the first place, consider the nature and most frequent seat of this first stage, and then, from the results of the preceding Part of this volume, point out what must occasionally be the consequences of its continuance; which we shall find is precisely what happens in those cases where, either from the too great severity of the offending causes, or the too little resistance in the organs concerned, this first stage runs the course which under either of those circumstances is unavoidable, where no effectual means are employed to prevent it.

The diseases I am about to consider, consist of three distinct stages: The first, merely in the affection of the parts on which the causes make their impression. The second, a state of general

nervous irritation induced by the continued causes of irritation existing in those parts, for the nervous system being a whole, if irritated in one part, is irritated in all; and the symptoms of this stage of the disease are infinitely varied, according to the tendencies of different constitutions.

The disease is now one of the whole system, wherever it may have originated, and the immediate cause of the sufferings is in the central organs of the vital system, on which all causes of irritation existing in that system necessarily make their impression. The long-continued irritations of the vital system have, as must at length happen, wherever they exceed the power of resistance in these organs, induced in them more or less functional derangement.

Under these circumstances they are no longer capable of supplying in due degree the healthy nervous influence, on which the assimilating and secreting functions depend. There is, therefore, throughout the system, a general tendency to failure in these functions; and if there be any of the vital organs from other causes more inclined to disease than the rest, this organ chiefly suffers, and, according to a well-known law of our frame, acts as an issue towards all others, and becomes the chief seat of the disease induced by the functional affection of the vital organs of the brain and spinal marrow.

Such is the third and fatal stage of the disease, and the reader will easily perceive why it is necessarily fatal, if the train of events which have led to it be overlooked. The original cause of irritation, although generally alleviated by the supervention of the new disease, still continues, still farther increasing the functional disease of the central organs; and the onlyc hance of safety is ascertaining the original affection, and removing it, which, for reasons which will appear, may generally be done even in this advanced stage, provided the last supervening disease has not produced actual change of structure.

We thus relieve the central organs from the cause which has impaired their powers, and afford the only remaining chance of

their regaining their healthy functions; and thus of relieving the organ last affected, in the affection of which the immediate danger always lies; and which is more pressing than the same affection arising from other causes, in proportion as the central organs, on which the structure as well as the functions of all vital organs depend, have been debilitated by the previous course of the disease.

The seat of the affection with which the diseases we are considering commence, rather than the cause which produces them, determines their nature; because they may arise from any causes which derange the functions of the parts concerned, and for a sufficient length of time maintain the derangement; and to such causes those parts of course are most exposed which are most exposed to causes of irritation, making their impression on the parts themselves, and the sympathies of which are most extensive. There are no organs to which these observations so extensively apply as the alimentary canal; yet causes of irritation chiefly operating on them alone are not frequently the source of those diseases, because their causes of irritation are generally of a temporary nature, and their most powerful sympathy is with the central organs of the sensitive, not those of the vital system. It now and then happens, however, that such is the result. I shall here give a concise account of such a case, on account of its rarity, and because it strikingly illustrates several important positions respecting the nature of this class of diseases.

The case I refer to was that of a lady who had for many years been exposed to the frequent recurrence of severe irritation of the bowels; yet this state of the bowels depended little on bilious derangement, as it is usually found to do. She did not labour under the diagnostic symptoms which always attend bilious complaints of long standing, and which I shall soon have occasion particularly to point out; and occasional mercurial doses never gave even the temporary relief they usually do in such cases, but only added to the irritable state of the bowels. Her attacks gradually became more frequent, till at length her strength was permanently

impaired. In a short time after this, she became the subject of an obstinate cough, which had never been the case at any former period of her life, although she was now between fifty and sixty years of age.

As she was past the time of life when a disposition to pulmonary consumption usually appears, and her family on neither side had been subject to this disease, the cough excited little alarm; but this, like her other symptoms, proved obstinate, and in a very short time after it had attracted serious attention, symptoms indicating diseased structure of the lungs appeared. Her debility was such that she became subject to sudden fits of syncope, and general anasarca supervened. She was now confined to bed, and died of one of the most rapid cases of pulmonary consumption I have witnessed.

The central organs of the vital system had been previously debilitated by the long-continued irritation of the alimentary canal, without any other morbid affection, although, as I have already had occasion to observe, the more evident sympathies of this canal are, from its great sensibility, rather with the central organs of the sensitive than those of the vital system. The relations were surprised that a person at her time of life, who had never shown the least disposition to such a disease, should have been carried off by a more rapid attack of it than they had ever known in any other case.

Here the central organs of the vital system were injured both by the direct effect of the irritation, and sympathetically by the effect produced on the central organs of the sensitive system by an affection of so extensive and highly sensible an organ, and of such continuance. By the previous disease, the system was in every way prepared for the result when the affection of the lungs supervened: the central organs of both systems had been strained, hence its rapid progress. Can any one doubt that if in this case the irritable state of bowels, which was supposed to be attended with no risk to life, had been relieved, the patient's life would have been saved? Here was a cause gradually and imperceptibly

undermining the powers of life, which was thought both by the patient, and those she consulted, to be of too little consequence to be seriously attended to.

In this lady's constitution there was so little predisposition to serious disease, except in the alimentary canal, that the lungs became the seat of the last supervening and fatal part of the disease, merely because they are the organs, from the nature of our constitution, most liable to change of structure.

Although the state of the alimentary canal is rarely the only source from which such diseases spring, in a great proportion of cases it powerfully contributes to their production, both by the causes of irritation existing in itself, and by its immediate influence on the organ which we shall find the most fruitful of all the sources of such diseases in this country.

The intimate connexion between the alimentary canal and the liver, the fluid secreted by which is at once the means of completing the digestion of our food and regulating the action of the intestines, greatly increases the influence of that canal throughout all parts of the system; and in particular from the peculiar sympathies of the liver with the central organs of the vital system; as, in its turn, the liver, through the alimentary canal, obtains an influence on those of the sensitive system; which, from its being so ill supplied with nerves of sensation, it only in a slight degree itself possesses.

The affection of the organ in which the disease we are considering originates, must have two conditions. It must belong to an organ little disposed to change of structure, and its immediate effects must not be such as directly threaten life. Without the former condition it could not be of so chronic a nature as to produce debility of the central organs, which, when it arises from such a cause, is always the effect of long-continued or very frequently-repeated irritation. Without the latter it could not be so lightly considered as it usually is, either by the patient or medical attendant, as at an early period to be often almost wholly disregarded.

The first points of consequence which present themselves, are to ascertain the organs on which, in such cases, the offending causes are most apt to make their impression; and the nature of the diseased state produced in them. If it can be detected and removed, all that follows is of course prevented. If the last stage has been allowed to appear, the safety of the patient, as I have just had occasion to observe, depends on distinguishing it from a case of original disease of the part now most prominently affected, and tracing the disease to its origin; because, unless the cause which has produced, and is *à fortiori* capable of supporting it, can be detected and removed, our means must either be altogether ineffectual, or afford imperfect and but temporary relief.

Of all our organs, the liver is the one which partakes most of the affections of the alimentary canal, and in a large proportion of cases supplies the source of many of its most powerful causes of irritation. I have, both in my Treatise on Indigestion and Gulstonian Lectures, entered at length into the proofs of the extensive influence of the liver, although an organ of little sensibility, on the sympathies of our frame; by which, more than any other cause, the consequences of its morbid states are regulated; a circumstance in a considerable degree depending on its intimate connexion with that canal, but in a far greater degree on the immediate connexion which exists between it and the vital organs of the brain, —a connexion far more powerful than exists between any other part and those organs. Even affections of the mind are capable, through the central organs of the sensitive and vital systems, of immediately deranging its function. Its chronic diseases cause melancholy, as the term itself expresses, and its acute diseases delirium, while the mind remains steady to the last in similar affections of all the other thoracic and abdominal viscera; for its influence is great on the central organs of the sensitive system, although it is so ill supplied with nerves of sensation; arising from the powerful sympathy which exists between the central

organs of the two systems, and the direct influence of its affections on the alimentary canal.

Thus it becomes the most fruitful source of the class of diseases we are considering. Owing to its little sensibility, its slighter and more habitual affections are often overlooked, while from its function regulating that of the alimentary canal, their influence is spread over the most extensive and sensitive surface. When to these circumstances its direct and powerful influence on the central organs of the vital system is added, we cannot be surprised at its effects, both in causing and regulating the course of disease.

It is a striking fact, and powerfully illustrates what I am now saying, that in sultry climates, where our sympathies are most, and for the security of health too active, almost all diseases, however they commence, terminate in disease of the liver; and even in temperate climates its general influence on all important diseases may be observed. We never see any serious disease in which its functions are not more or less deranged; but here its affections are less observed, and, if I may so speak, less monopolise disease, because they are not, as in sultry climates, disposed to run to deranged structure, and disease of structure is more powerful than that of function, in relieving all concurring diseases.

If these facts be such as here stated, and to the well-informed and experienced physician they are too notorious to be questioned, —I say, if such be the facts, and we had understood the nature of the diseases which form our present subject, we might have foretold that the liver must perform an important part in their production; and how amply would such a prediction have been confirmed by actual practice, did my limits admit of it, I could prove by reference to many diseases of the class we are considering.

Another circumstance which renders the states of the liver of the first importance in these kingdoms is, that in them derangement of its function is of all diseases the most hereditary. We often

find whole families at the same time affected with it, and its consequences. Thus it proves the most fruitful source of the diseases we are about to consider.

Thus even the slighter affections of this organ, although themselves unattended with danger, become of the first importance, and have not met with the attention they deserve. I shall here enter particularly into their nature and diagnostic symptoms, and afterwards state the facts which prove their immediate connexion with many of the most fatal diseases to which we are subject.

As we have not been aware, as appears from the preceding Part of this volume, that the organs of the leading power in the vital system reside in the brain and spinal marrow, and that any cause disturbing the functions of these organs, supposed to be those of the sensitive system alone, may directly influence any other vital organ, and, on the other hand, that the state of any of the latter is capable of directly influencing the vital organs of the brain and spinal marrow—I say, not being aware of any of these circumstances, we could not be prepared for the observations I am about to make; but having before us the facts on which these positions are founded, if we find at the same time that the daily phenomena of disease prove that, on the one hand, any cause which permanently debilitates the brain or spinal marrow, and still more both of these organs, in the same proportion, tends to impair all the vital functions, and at length may derange the structure of any of their organs; and, on the other, that a permanent disease of any vital organ, however inconsiderable, directly tends to debilitate the vital organs of the brain and spinal marrow; I say, if such be both the physiological and practical facts, it is doubly proved that a fruitful source of disease has been more or less obscured by the physiological error. Such are the general principles which cause the morbid affections of the brain and spinal marrow jointly to be so frequent and important, in their origin often so obscure, and often so fatal in their termination.

We are thus led to inquire into the causes which tend to debi-

litate the organs on which the maintenance of all parts of our frame depend, to endeavour to trace their first beginnings, and to inquire into the means of arresting a change which necessarily tends to a fatal issue.

We have seen that of all our organs the one which, both in consequence of its own sympathies, and those of the organs with which it is connected, most influences the vital organs of the brain and spinal marrow, is the liver. Certain affections of this organ may be regarded as the endemic of these kingdoms, arising, as I have already had occasion to observe, partly from its decidedly hereditary nature, and partly, we have reason to believe, from the changeable nature of our climate. So frequent are the less severe functional affections of the liver, that if any three persons we meet be examined, at least in one, perhaps in two of the three, it will be found more or less functionally deranged, that is, out of any three, one or two will be found to be more or less what is called bilious; a term which the little attention which has been paid to the slighter affections of the liver has caused to be used in a very indefinite sense, so much so, that, in attempting a more correct view of the beginnings of the diseases we are considering, it will be necessary to dismiss this term, which, we shall find, includes affections very different in their nature, and use less equivocal language. I am here, then, to consider those slighter disorders of the liver, which, however in themselves free from risk, may, like any other chronic affection of a vital organ of long continuance, in process of time, through the vital organs of the brain and spinal marrow, lay the foundation of any one of a large proportion of the most distressing and fatal diseases to which we are subject.

This gives to them an importance which does not at first view seem to belong to them, for the affection of the liver of which I speak, never, according to my experience, in temperate climates, independently of other concurring causes, injures its structure. I have never, on the one hand, seen organic disease of the liver, produced by the disease known by the name of bilious com-

plaints, where it depended on an original affection of the liver, namely, the state of the liver I am about to describe, even where it had lasted for the greater part of life; and, on the other, in all the cases of disorganised liver in which I have been consulted, they have always been found to arise from other causes. I am well persuaded that what is accurately called bilious complaints never in this way and in this country lead to danger. It is in the way above pointed out, that they ever produce a fatal result. But for every case of disorganised liver, I believe I speak within bounds when I say, we see hundreds of more or less functional derangement of this organ.

I am now to present to the reader a careful consideration of the nature and tendencies of this disease, and the treatment I have found most successful in removing it, and finally correcting its tendency to return. I shall in the first place point out its diagnostic symptoms, by which we shall, on the one hand, clearly perceive what constitutes the disease; and consequently be able accurately to define its nature, that is, to trace the defect on which it immediately depends; and thus distinguish it from other diseases, which it so nearly resembles, that we have at present no diagnostic applicable to all cases, some of a very different nature, and consequently requiring very different, and even in some respects opposite, means of treatment, being known under the vague denomination of bilious complaints.

By this means we shall be enabled to judge with accuracy of the degree in which its cause exists in each particular case, and, from time to time, of the effects produced on it by the plan of treatment adopted; points greatly required in a disease which often produces such disturbances in the nervous system, that both the sufferings and the danger are generally proportioned more to the excitability of this system, than to the degree in which the cause of the disease exists; a circumstance which we shall find essentially influences both the prognosis and treatment. Having pointed out the diagnostic symptoms and their more immediate

attendants, I shall then consider the consequences most to be apprehended from the continuance of the state they indicate, and the symptoms which point out the approach of such consequences.

Of the Nature and Diagnostic Symptoms of what are called Bilious Complaints.

The liver, when healthy both in function and structure, lies entirely under the ribs. When the structure is disordered, it is generally, and sometimes greatly, enlarged, and therefore descends below the ribs; less frequently it is contracted in size. To such cases the present treatise does not extend. They are well understood, and from the nature of the symptoms cannot at any period be disregarded.

It is the functional derangement of the liver alone to which I wish to call the reader's particular attention, and after it has become habitual. The liver is then found uniformly so distended from the accumulation of bile in its ducts, as to descend below the ribs, and it is easy to ascertain the extent to which it does so, because, although there is no hardened edge to be felt as in the case of enlarged indurated liver, there is a greater degree of fulness than in the corresponding parts of the left side as far as the liver descends; and the patient feels the breathing more affected by the pressure on the full parts, than by similar pressure on the corresponding parts of the left side; while, as I have ascertained, both alone and with other physicians, in a perfectly healthy subject no difference can be detected, either by the feelings of the patient or the hand of the physician, between the corresponding parts of the two sides. The pressure being made through the integuments and muscles, the difference is not sensible to the hand of the physician, when it is only the extreme edge of the liver which comes below the ribs; but however little it is below them, the pressure will always cause the patient to feel the breath more affected than on the other side, because it will still more or less

press the liver upwards against the lungs; whereas when the liver is wholly under the ribs, its natural position, no effect of this kind can take place. The standing position is the most favourable for the examination, because it is in this position that the distended liver approaches nearest to the parietes of the abdomen, and as there is no hardened edge, the degree and extent of which we wish to ascertain when its structure is disordered, any action of the muscles which in this position takes place, is no impediment towards ascertaining with accuracy the degree to which the liver is distended, and when it has regained its healthy size, and consequently wholly retired to the position which a healthy liver always occupies.

But the liver is not the only bowel that is morbidly distended when the function of that organ is impaired.

As the morbid distension of the liver arises from the bile to a greater or less degree accumulating in and distending its ducts, it cannot under such circumstances, with regularity and in either the proper quantity or quality, (for it always acquires morbid properties when too long detained in the ducts of the liver,) be regularly supplied to the duodenum; where it mixes with the contents sent from the stomach; in its healthy state completing, as I have already had occasion to remark, the process of digestion,[1] which is always imperfect if a due supply of healthy bile is prevented, and giving to the contents of this bowel the properties required for maintaining its healthy action.

Thus it always happens, that when there is a morbid accumulation of bile in the liver, there is more or less a morbid accumulation of food in the duodenum; the action of the bowel being impaired in proportion as its contents deviate from those which maintains its healthy excitement. They are always transmitted, but this is effected less perfectly, and consequently some accumulation gradually takes place in it. I have never seen one instance of an habitually distended liver unaccompanied by an habitually distended duodenum, which is easily ascertained by the in-

[1] See the experiments of Sir Benjamin Brodie on this subject—Brand's Journal, (Royal Institution).

creased fulness felt by the hand on pressure on the region of this bowel, and the pressure having the same effect on the breath as that on the region of the distended liver, but less in proportion as the distended bowel is less firm and at a greater distance from the lungs; and such is the enlargement occasioned by the distended state of these two organs, that when their proper function is restored, if the bilious complaint has been of long standing, women, although no thinner, generally find it necessary to lessen the width of their stays from three to five inches.

Now these two states of distension are the diagnostic symptoms of the disease I treat of; and however much other cases may resemble it, where there is no distension of these organs, I have uniformly found that the state of the liver has had no share in producing the disease; and that the treatment adapted to bilious complaints, properly so called, is of no service, and consequently injurious, except as far as may be necessary to relieve bilious symptoms occasionally attending them. To prevent all ambiguity, I shall call the disease before us distended liver. Such, then, are the diagnostic symptoms of the common bilious complaint of these kingdoms, which is so common, that it deserves the name of their endemic.

We are now to consider the more remote and less uniform consequences of a permanently distended, although, as far as structure is concerned, a perfectly healthy state of the liver. In many instances, the consequences of the distended state of the liver continue to be of trifling importance; so much so, that the subject of them considers his general health good, and such is the case. In the greater number of instances where the liver and duodenum are more or less morbidly distended, the only inconveniences are a tendency to flatulence and languid bowels, sometimes occasional heart-burn, symptoms that are generally easily relieved, and, unless in extreme cases, little if at all affecting the general health; and in this state I have known many remain for the greater part of life. The symptoms which necessarily belong to a distended state of the liver and duodenum are

much less important than, considering the nature of these organs, we should be inclined to suppose.

The most frequent of the occasional effects of a distended liver is tenderness on pressure in the region of the pylorus, where the morbid fulness is chiefly perceived; a symptom we shall find of great importance in the treatment, rendering it both more complicated and tedious; and arising from a chronic inflammatory state of the pylorus, more or less extending to parts in its neighbourhood. I have often found the pylorus in those who had long laboured under indigestion, as red as if it had been affected with acute inflammation. It is evident that while other parts of the stomach are exposed to the undigested contents which happen to lie next them, the pylorus is exposed to the whole. All must pass by this orifice.

I have never seen an instance, however, in which this tenderness was attended with pain or any other symptom of acute inflammation. The patient is never sensible of its presence, till pointed out by the effect of pressure on the part. The pains occasionally attending distended liver appear to be all of a spasmodic nature, and generally referred to the stomach.

The inflammatory state of the pylorus spreads to the left edge of the liver, and in some cases to a considerable distance along this organ, as may be ascertained by pressure on that part of it which has descended below the ribs. More rarely there is a sense of tenderness also in the region of the distended duodenum, but this, unless very considerable, requires but little attention, as it almost always disappears when the tenderness of the pylorus and liver is relieved.

Of such consequence is this tenderness in the treatment, that I cannot recollect a single instance in which the liver has ever retired into its proper place till the tenderness was subdued; the removal of which, we shall find, forms an important part of the treatment, its presence always more or less, and often greatly, protracting the cure. Being of an inflammatory nature, it tends to bind up the surfaces, whereas the object of the treatment is to

relax them, and cause a free flow of the secretions. The tenderness accompanies about two-thirds of the cases of distended liver, and appears to depend on the inflammatory habit of the patient, those who are most disposed to it being also most disposed to feverish heat, more or less tendency to which accompanies a large proportion of cases, being induced by the various irritations which attend the more severe cases. Such may be regarded as the first stage of distended liver, which in those of strong nerves, we have seen, will sometimes continue for the greater part of life without assuming any more serious form.

When it makes the next step, the patient's state is essentially altered.

On the first Stage of the Debility of the Central Organs of the Vital System belonging to the third division of the present Section.

Although in those of strong nerves a distended state of the liver may remain for an unlimited time, whether attended with pain on pressure or not, without producing any serious consequences; in many, this and the distended state of the duodenum, which we have seen always, and indeed necessarily, accompanies it, gradually begin to fret the nerves of these organs. As soon as this takes place, the nature of the disease is altogether changed.

The vital system is a whole, which if fretted in one part is fretted in all. The disease thus becomes one of the whole system, and the symptoms which then arise are of infinite variety, varying with the endless variety of constitutions, and agreeing only in this, that they produce, more or less in different cases, a disturbed and restless state of the whole system, for the sensitive doubly partakes of the disease through the central organs of the vital system, and the nerves of the original seat of the disease. The symptoms, at first, are in general only occasional; but if no effectual means of obviating them be employed, almost always

more or less increase in frequency, and some degree of them becomes permanent. The patient now feels relief necessary, and it is under such circumstances that he usually applies for medical assistance.

In some instances the degree of suffering is very great; and what particularly points out its nature is, that this stage of the disease is often hastened by some other cause of nervous suffering concurring with that of the distended organs.

Anything which greatly affects the feelings, or any effort which for a considerable time strains the mind, may, in concurrence with the disease, hasten on the stage I now speak of, or produce it when it would not otherwise have occurred, for the sympathy between the vital and sensitive organs, we have seen, is mutual. It is not uncommon in such cases to hear the patient say, the fretting which caused my present distress was what I suffered on such an occasion, or it came on after I had strained my mind by too severe application. Being further questioned, he will recollect that he had previously some symptoms of indigestion, or of other chronic disease. When the first of these has been the case, I know, before I make the examination, that the liver is morbidly distended, and I never, under such circumstances, once found it otherwise.

But whatever be the sufferings induced by this state of the disease, it is still free from immediate danger, and, when it takes no other step, very severe suffering may continue for many years, as I have before observed, without any symptoms of danger supervening. The only exception to this observation is, that those in this stage of the disease are liable to occasional attacks of bilious, often terminating in nervous fever, which, however, does not frequently occur, and is rather a tedious than a dangerous disease; but the next step in the progress of the disease before us always leads to danger, and, in many cases, of the most pressing nature.

On the last Stage of the Debility of the Central Organs of the Vital System belonging to the third division of the present Section.

We have seen from the facts detailed in the second part of this treatise, that the brain and spinal marrow, so far from being merely organs of the sensitive system, include the organs which may justly be termed vital, those which regulate all the more complicated functions on which the growth and well-being of every part, both sensitive and vital, depend. It further appears from the facts there detailed, that the brain and spinal marrow are the only organs employed in the formation of the nervous influence, the agent by which all the functions of the nervous power, properly so called, whether belonging to the vital or sensitive system, are affected; the nerves, ganglions, and plexuses being only the means of conveying that influence, and, in the vital system, of combining the power of the vital organs of the brain and spinal marrow, for the due performance of the various processes of secretion and assimilation.

If such be the facts, it is evident that any cause impairing the nervous functions, unless it be a local cause impeding the passage of the nervous influence through particular nerves, must have this effect through the brain or spinal marrow, or both. Thus all general failure of the nervous functions is referable to a debilitated or other morbid state of one or both of these organs; and we have seen it must be equally of both, when arising from causes affecting any distant vital organ. But as we have found that all the functions of secretion and assimilation depend on the vital organs of the brain and spinal marrow, the healthy action of these organs cannot be impaired without a tendency to failure in functions, which endangers all our organs, vital as well as sensitive. Such is the state induced by what has been called bilious complaints—that is, by the distended state of the liver and duodenum, or long continued chronic disease of any other vital

organ, as soon as it has produced a general and continued fret of nerves; and, under such circumstances, any organ which happens to be most inclined to disease, either by the original laws of our frame, previous disease, or any other cause, will suffer most; and by its suffering, according to a well-known law of our frame, divert the effects of the general derangement from other parts.

The symptoms thence arising are the last train of morbid affections produced by a permanently distended liver, and the only way in which it proves fatal in our climate. It is in this last stage alone that the danger is immediate, but here it is often much greater than at first view appears.

As in other diseases not arising from mechanical violence, the derangement of the organ last affected is at first only functional. But being now the most prominent part of the disease, it obscures the preceding symptoms, which are often in a great degree overlooked; and the more readily, that the supervening disease generally more or less relieves that which produced it; and if the practitioner be not aware of the nature of the state of the system which has previously debilitated the powers of life, he cannot be prepared for the rapidity with which the functional in such cases is often followed by structural derangement.

The disease I have been describing, we have seen, consists of three distinct stages; the first merely a distended state of the liver and duodenum, and the slight derangement chiefly of the alimentary canal, which accompanies them; free from danger, and causing but occasional, and, in general, trifling local uneasiness.

The second stage is distinguished by a general fret of nerves having supervened, causing varied and often severe suffering, but still without immediate danger.

The third and last stage is characterised by an affection of some other vital organ. Here the suffering may be little, if at all, increased, and is sometimes alleviated, because the supervening disease often relieves many of the previous symptoms; but the danger, for the reasons just mentioned, is often great, of course

greater or less according to the nature of the organ on which the disease fixes, the severity of the symptoms which last supervene, and the degree in which the vital organs of the brain and spinal marrow have been previously debilitated.

At different times of life different organs are most disposed to disease: hence it is that pulmonary consumption is frequently the fatal termination of distended liver in early life, palsy and apoplexy in late life; and wherever, after the fret of nerve has come on, there is any disposition to disease in any particular organ, the state of that organ should be watched with care.

Such are the nature and frequent course of the disease I have been describing, or of any other chronic disease of a vital organ of long continuance. We have seen an instance of its arising from such an affection of the alimentary canal alone, and running through all its stages to the last fatal and rapid termination. The course of the distended liver is not peculiar to it; but as it is the most frequent of all such diseases, it is the most frequent source of the succession of the diseased actions which has been described; and although it but occasionally proves formidable, if we compare the frequency of the formidable cases with that of other serious diseases, they must be regarded as among the most frequent of such cases: and, from the whole of my own experience, I feel no hesitation in saying, that if, in these kingdoms, the whole course of functional derangement of the liver and its consequences be fairly estimated, they will, at the present period, be found eventually the most fatal of all our diseases. When to this we add the frequency of slighter cases, and observe how much other diseases are modified by the influence of the liver, in consequence of its sympathising, to a degree that no other organ does, with the central organs of the vital, and, through the alimentary canal, with those of the sensitive system also, we have a correct view of the great importance of the liver both in creating and modifying disease; and need not be surprised at the fact above referred to, that in the countries where these sympathies are most active, all diseases, in whatever way they commence, if long pro-

tracted, terminate in affections of this organ,—a fact strikingly illustrative of all I have said on this part of the subject.

How comes it, it may be said, if such be the facts, that they have demanded so little of our attention. The facts have not been overlooked; they are well known, and will at once be generally acknowledged. The inferences from them are what has been overlooked. They have not been so connected in the mind as to enable us to arrive at them, because there was a necessary link wanting in the chain of our reasoning. There has been all along, as appears from what is said above, a general feeling in our profession, that the brain and spinal marrow are organs of the sensitive functions alone, and we have therefore looked to them as influencing disease no farther than it is found to be influenced by these functions. We were not aware that nervous irritation, long supported by a cause however in itself of little importance, was tending *directly* to impair the vigour of the organs of the leading power in the vital system, with the failing power of which the vigour of all other vital organs must fail. We are thus enabled to compare the ultimate with the immediate effect of causes which, at first view, appear so trivial; and when the former show themselves, by tracing them to their source, clearly to perceive the course we must pursue to obviate them: for it will ever be vain to attempt the cure of a disease, if the cause which produced it be allowed to operate, particularly when the cause exists in our frame, and from its nature is by habit, except as far as it is relieved by the supervening and more fatal disease, every day that it is overlooked, gaining additional power.

Such are the circumstances which give to functional disease of the liver an importance which at first view appears little to belong to it, and which, in an early stage of my practice, turned my mind to a careful observation of its effects; for it is now nearly five-and-twenty years since I was aware that the leading power in the vital system resides in the brain and spinal marrow, and in the year 1815 laid the facts on which that opinion was founded before the Royal Society, which did me the honour to publish them in their Transactions for that year.

The intelligent reader will perceive by what has been said, that the importance of the liver in the cases before us does not arise from the frequency of this affection alone, but on this, together with the liver being possessed of those properties which more than any other organ suit it for laying the foundation of such a train of disease. It is of all our vital organs the most subject to chronic debility, and, with the exception of the brain and spinal marrow themselves, of more extensive influence with respect to other vital organs than any other of our frame, and the least of all those organs, in such climates as our own, subject to change of structure from such functional affection as is capable of producing the effects in question. Thus we find it possessed of all those qualities which we have seen tend, by its chronic affections, to produce a debilitated state of the central organs of the vital system.

On the supervening affection which characterises the last stage of the disease, I am now to make the necessary observations.

As, in treating of the first stage, I selected the distended liver as the most frequent cause of the class of diseases we are considering, and the observations it was necessary to make respecting it are more or less applicable to any other similar affection occurring under the some circumstances, and having the same tendency; I shall in like manner, in considering the local affection which characterises the last stage of the disease, select that in which it most frequently terminates in these kingdoms—pulmonary consumption; and the observations which apply to it will be found more or less applicable to any other affection of a similar nature occurring under the same circumstances.

Another reason for the selection of this disease is the vast difference in the nature and, consequently, the tendencies of the original and symptomatic affection of the lungs, which has attracted too little attention.

The circumstances which in pulmonary consumption render

the distinction between the original and symptomatic disease doubly important, are, that the former is often a fatal disease from the commencement—so little is the power of our remedies over the original affections of the lungs; while, in all cases, the latter can be arrested in its early stages, and generally in all stages previous to the structure of the lungs being actually diseased, which in most cases occurs at a later period of the symptomatic than idiopathic affection; and for every case of the original disease in these kingdoms, we meet with at least twenty, but, according to my experience, many more of the symptomatic.

It also deserves attention, that the latter is that in which the hereditary predisposition is by far the most powerful cause of its greater frequency. I have never in a single instance known many members of the same family fall a sacrifice to the original disease, while I have seen almost whole families swept off by the symptomatic form—that form, although as certainly fatal as the other if the structure of the lungs has been allowed to become seriously diseased, but which, in its early stages, is always curable. A most gratifying proof of this is, that I have had the satisfaction of seeing in many families, in which one after another had fallen a sacrifice to symptomatic phthisis, its ravages wholly and finally prevented, and that by means of easy application; so that there is reason to believe that when the proper treatment becomes general, as this form of consumption is by far the most frequent, the disease which at present destroys annually so many thousands in these kingdoms will be comparatively rare.

It is a striking fact, and shows how essentially different is the nature of these cases, that in the practice of more than thirty years I have never seen the two forms of the disease in the same family.

I can even go farther than this, for I cannot recollect one instance of the original disease of the lungs having appeared in two individuals of the same family. According to my experience, it cannot therefore be regarded as an hereditary disease. Nor, according to my experience, can the common form of the

disease be strictly regarded as such, because, as it only appears in the symptomatic form, and is uniformly averted by preventing the original disease, which therefore appears essential to its production, the disease itself hardly deserves the name of hereditary.

There can be no doubt of a weakness of lungs being hereditary; but if the circumstances just stated be correct, and I have witnessed them too often for a doubt to remain on my mind, this weakness of lungs will only produce phthisis, when other circumstances concur with it; and as these circumstances are under our control in this form of the disease, although, when the structure of the lungs has been allowed to become seriously injured, it is as fatal as that which originates in the lungs themselves, I believe it may almost always be arrested at an early period.

My experience has furnished me with but one cause which is beyond our control, namely, when the cause of the fret of nerve, which conspires with the hereditary debility of lungs in producing it, arises from a settled grief; in consequence of which, from the sympathy of the central organs of the two systems, the affection of those of the sensitive spreads to the organs of the vital system.

It will appear from all that has been said, that it is the functional, not organic, affection of the liver, which lays the foundation of the symptomatic phthisis of which I speak. The organic affection of the liver is too evident a disease to be disregarded; and when, as often happens, it terminates in disordered structure of the lungs, this disease is to the most careless observer evidently symptomatic.

This circumstance has tended to confirm the error which has prevailed; because, according to a law of our frame which I have had frequent occasion to refer to, the original disease, when only functional, is generally relieved by the secondary disease, even when it also is functional; but when the latter has become structural, it never fails to relieve, and very often wholly removes, the functional disease from which it sprang. From this cause many

comparing the appearances with what they find when organic disease of the liver produces disease of the lungs, have been confirmed in their erroneous view of the case, by finding, on examination after death, that the liver bore no marks of disease.

We have a memorable instance of this error in the post-mortem examination of the late Dr. Currie, a man both of talents and correct observation, and who had long laboured under bilious complaints, and regarded all his complaints as originating in them. But those who made the post-mortem examination having reported the liver to be quite sound, but the structure of the lungs destroyed, it has been generally supposed that he had mistaken his own case; he could not have been mistaken that he had been long distressed by bilious irritation; and we can have little doubt the mistake imputed to him was the consequence of the fact just mentioned.

Unless the symptoms of disordered structure of the lungs are unequivocal in the cases we are considering, however in other respects alarming the symptoms, experience has taught me never to despair, unless all morbid distension of the liver having disappeared, it has retired into its proper position under the ribs; for I have in a few instances seen recovery by the means I am about to refer to, where at first view the case appeared to be hopeless. But the affection of the liver being completely removed, without having afforded relief to the pulmonary symptoms, I have, as stated in my Treatise on Indigestion, uniformly found a fatal symptom.

Although, in general, the symptomatic phthisis is slower in running to diseased structure than the idiopathic, when, in the case before us, as I have already had occasion to observe, the fret of nerve has not been such as greatly to debilitate the central organs of the vital system; when this is the case, it rapidly assumes an alarming character. As in all similar cases in this country, the original disease which concurs with the hereditary debility of lungs in destroying their structure, being necessarily a chronic disease, the secondary, except in the more severe cases, partakes of the nature of the original disease.

I have already had occasion to observe that where there is no peculiar weakness of lungs or of any other vital organ, they often suffer merely from their being the organ, particularly in the period of youth, by the constitution of our frame, most inclined to disease; an observation well illustrated by the tendencies of advanced life, in which the brain becomes the organ most liable to disease; and here, we have seen, the same cause terminates most frequently in palsy and apoplexy, and I can say from long experience that it is among their most frequent causes.

On reviewing all that has been said, it appears that in our own climate we can trace the same extensive influence of the vital sympathies of the liver which we have seen, from the more active state of our sympathies, are so much more remarkable in sultry climates; and where their effects are rendered more powerful by the greater tendency in such climates to organic disease of this organ, which we have seen causes it, as it were, to monopolise the diseases of such climates, particularly of the East Indies; and at once explains to us the well-known risk to Europeans in there indulging in the free use of fermented liquors, which in all climates is a powerful cause of disorganised liver. It is fortunate for the inhabitants of our own climate that the disease of this organ, so prevalent in it, is of a milder nature, and I believe in those of regular habits always removable, even after its more serious consequences have begun to show themselves; and were this the proper place for such a discussion, it would be easy to show that the same extensive sympathy with other vital organs appears in the symptomatic as in the original affections of the liver; for there is, even in this country, no serious disease in which its state can be safely disregarded. The same cause which renders it liable to partake of other diseases, enables it to influence their course.

On the Treatment of the first Stage of the Debility of the Central Organs of the Vital System, belonging to the third division of the present Section.

It is, we have seen, in the second stage of the disease we have been considering, that the vital organs of the brain and spinal marrow begin to suffer.

I shall here, as in considering the symptoms and nature of the disease, take as an example the most common case, that arising from distended liver.

Without entering fully into the treatment of this stage of the disease, for which I beg to refer the reader to the seventh edition of my Treatise on Indigestion, I shall only endeavour to point out the causes which have hitherto rendered the treatment in it so inefficacious; for the usual plan, in the first and second stages, after a certain degree of relief is afforded, is to tell the patient to attend to the rules of diet we lay down, regulate the bowels by the means we have pointed out, think as little as possible of his complaints, and have recourse to a dose of calomel when he feels more indisposed than usual; and so he is dismissed, more or less an invalid for life, even if the third stage does not supervene. I have said, however, that under such circumstances a cure may almost always be effected. I shall here point out what, as far as I can judge, have been the causes which have prevented the permanent cure, in all cases of long standing; and the principles on which repeated experience has assured me that such a cure may be effected.

It appeared to me that a principal cause of our failure was trusting alone to doses of mercury, the only medicine on which we can rely in affections of the liver, of however slight a nature, of such amount as could only be given at considerable intervals, that of two or three days, without rendering the effects of the medicine as pernicious as the disease, or more so; in consequence of which, the bile always more or less accumulating in the interval

in the debilitated liver, and the effect of our doses being thus only temporary, the permanent cure was impossible. Another circumstance which, of itself, would have rendered a permanent cure, in many cases of long standing, impossible, even under the most efficient administration of mercury, is, that we overlooked a consequence of the long continuance of the bilious complaint and the impaired function of the stomach, which, we have seen in the majority of cases, more or less attends it, the continuance of the irritations of indigestion producing an inflammatory state of the parts most exposed to them, which I found, from post-mortem examinations, first takes places in the pylorus, (which, in those who had long suffered from bilious complaints, was often found as red as if it had been actively inflamed,) and spreads more or less to neighbouring parts, and particularly to the left edge of the liver, and less frequently to other parts of the stomach.

This causes a tenderness, on pressure, to be felt in the region of those parts, and prevents the proper effect of our medicines. In the least inflammatory habits this tenderness does not always occur; but I can say, from extensive experience, that where it does, which, we have seen, is in a large majority of cases, its continuance obviates the effect of our remedies, and renders the removal of the disease impossible. All inflammatory tendency binds up the surfaces, whereas the object of the treatment is to relax them, for the bile will never flow with freedom if the surfaces in general are bound. A free state of the skin and bowels is requisite to the free action of the liver. Such is the power of its sympathies, that the proper treatment, particularly of its more chronic affections, includes an attention to the state of the general habit, and particularly to that of the organs with which it is most intimately connected.

With respect to the first of the foregoing causes which have rendered our practice in distended liver, if of any standing, so inefficient, it occurred to me, about twenty years ago, that as we cannot give the usual mercurial doses except at long intervals,

during which, in such cases, their effects are in a great degree obviated, some means must be found to maintain these effects during the intervals, before we can make effectual progress in removing the disease.

I tried dandelion, ipecacuhana, and other means, which have some power in exciting the liver, but none had any effect at all to be depended on, till I had recourse to minute doses of mercury, given at intervals of eight hours. The dose I found to answer the purpose was either the twentieth part of a grain of calomel, or half a grain of blue pill, which is equal or nearly equal in power to it, according as the one or other best suited the constitution. The minute dose of calomel is most aperient, but, on the whole, I have thought that the blue pill is the best alterative.

There are many circumstances to be observed in their employment, which have gradually shown themselves, and many of which are stated in my Treatise on Minute Doses of Mercury. Their good effects, however, are not to be wholly ascribed to their keeping up the effect of the larger dose, which must always be continued under their use, for they have little effect in enabling the liver to discharge the accumulated bile. In proportion as this becomes less, the larger dose may be employed at longer intervals, and in all cases it must be so carried off as to prevent its entering the system.

It is a law of our frame, we have seen, that although, in the sensitive system, every stimulant, however slight, is followed by corresponding exhaustion, in the vital system this is only the case with stimulants which exceed a certain power.[1] Thus it is, that in this system whatever produces the stimulant effect within that range in any vital organ, supports its function without any degree of subsequent depression, and thus acts as a tonic on that particular organ. I have found that such is the effect of the minute mercurial dose. While the excitement of the larger dose is followed by a more languid action of the liver, the minute dose, supplying a constant and gentle

[1] Philosophical Transactions for 1836.

stimulus within the limit which produces coresponding depression, acts as a tonic, and the organ gradually and uniformly recovers its power under its use; and the whole system, the debility of which depends on that of the liver, experiences a corresponding effect. And I can say that I am unable to recal one case where there was no disease but the bilious complaint, however long it had lasted, and the patient did not become tired of the means,—for in long-protracted cases the recovery is necessarily slow—in which he was not restored to permanent health; and I can at this moment point out cases, which had lasted for more than twenty years, in which the patient for many years past has enjoyed uninterrupted health. As the minute dose alone is allowed to enter the system, none of the bad effects of a course of mercury ever attend the treatment.[1]

With respect to the tenderness on pressure of the parts chiefly concerned in the disease, the only means of its removal, which I for a long time employed, was blood-letting from the parts of the skin in their immediate neighbourhood. The patient is never sensible of this tenderness, as it occasions no uneasiness, except on pressure in its seat; it is therefore always overlooked, if the physician does not detect it by an examination of the parts affected. Often-repeated local blood-lettings are frequently required for its removal, so frequent in some cases, that I found it necessary to look out for other means of its removal, and was so fortunate as to find the means I first had recourse to, even more successful than I expected; for I have not only never met with one case in which the tenderness on pressure resisted it, but, in addition to this effect, it has always greatly contributed towards removing the morbid fulness of the hepatic region, by decidedly promoting the return of the liver to its healthy position under the ribs. The remedy I speak of is a seton placed in the region of the pylorus, that is, in the original and chief seat of the tenderness on pressure. Nor is this so troublesome a remedy as at first view it may appear, because it is

[1] See my Treatise on Minute Doses of Mercury.

never necessary to allow it to remain long enough to form such a habit as makes any precaution necessary in removing it. The following is the practice I now pursue respecting this symptom.

In some, in whom the tendency is slight, it yields now and then to the first doses of calomel. This is rare. More frequently to a few repetitions of local blood-letting from the region of the pylorus. When it resists these means, I advise a seton to be placed in this region, from which I have hitherto always experienced the effects above mentioned.

Although the tenderness never assumes the form of active inflammation, it is sufficient to bind up the surfaces; and as our means must always be of the gentlest nature, (for what is called a course of mercury is capable of causing, and cannot therefore remove the disease,) their effects are easily counteracted. Along with the foregoing means some stomachic medicine is proper; for grateful as mercury is to the liver, to the stomach and bowels it is more or less offensive; but tonics can seldom be employed without injury, and never till all inflammatory tendency is subdued.

When all disposition to tenderness on pressure is removed, or, in cases where it has never supervened, if the distended state of the liver does not readily yield to the foregoing means, great advantage is obtained by combining with them the use of voltaic electricity employed in the manner and with the precautions I have pointed out in the seventh edition of my Treatise on Indigestion.

Such, with the means of relieving occasional symptoms, which are very various, depending on accidental causes and peculiarity of constitution, is the general outline of the treatment which I have found successful in effectually removing old bilious complaints; which, although themselves unattended with danger in this country, where the liver never runs to disorganisation from this cause, except in drunkards, have too little attracted the attention of the medical profession.

It is hardly necessary to add, after what has been said, that their effects are very various. They may, after they have induced a fret of nerves, terminate in a fatal disease of the head, chest, or abdomen, according to the tendencies of the particular constitution, the period of life, and the circumstances in which the individual happens to be placed; the principle of the treatment being then a combination of that of the original and supervening diseases; for the latter of which I must refer to the works on those diseases.

In all serious diseases, we should carefully determine, from an examination of the state of the digestive organs, and the history of the case, how far the irritations of these organs have had a share in producing the attack the patient labours under; because if this has been the case, and the disease of those organs, even although in a mitigated form, remains, the removal of the more prominent disease is impossible, unless that in which it has had its origin be detected and removed.

Thus I have found, in many both acute and chronic cases, an examination of the region of the three great digestive organs, the stomach, particularly the pyloric region of this organ, liver, and duodenum, of as much consequence as that of the pulse, and often more so. For many years I have never failed to have recourse to it; for even where such affections have no concern in the cause of the disease, they often, as above stated, from the extensive sympathies of these organs, supervene in it, and always influence its course, and ought, if present, to influence the treatment.

In by far the majority of cases in which my opinion has been requested, in consequence of their unusual obstinacy, I have found a distended state of the liver the cause of the obstinacy, and, on its being relieved, the disease has yielded to the usual means.

If there be any truth in the preceding observations, and they are equally founded in the general laws of our constitution and practical experience, how erroneous must that treatment be, by which all cases of the same disease of the same organ are often

treated in the same way, whether arising from a cause immediately influencing the organ itself, or one which has made its first impression on another part of the system; without correcting the effects of which it is impossible to remove the more prominent disease: for a disease will, *à fortiori*, be supported by the continuance of the cause which produced it. The correctness of this observation is strikingly illustrated by that which, on account of its eminence and frequency, I have chosen as an example of the last supervening disease; often even to the degree, if the structure of the lungs be still entire, I may say, of almost certain safety, instead of no less certain death.

SECT. IV. *On a certain state of Dyspepsia and habitual Asthma.*

THE diseases which form the title of this section, arising from a defective action of secreting surfaces, depend on a weakened power of the central organs of the vital system, in which I have found voltaic electricity the only effectual means of relief.

The first is a state of general debility uncharacterised by any particular symptom, generally the effect of the long-continued irritations of indigestion.

Of the latter, which, as far as I know, had not been distinguished as an original disease, the reader will find a particular account in a paper I presented to the Royal Society, and which is published in the Philosophical Transactions for 1817, in which is given the result of the treatment of about a hundred cases of what I have called habitual asthma.

Most of these were treated in the course of several years, in a public establishment, the County of Worcester Hospital, where the disease among the sedentary china manufacturers is of very frequent occurrence, and in the presence of many observers. Some had been obliged to give up their employment in consequence of this disease, for no other means that were tried gave any effectual relief; and I do not recollect one instance in which they were not enabled to return to it by the use of voltaic electricity.

The disease consists merely in a constant state of oppressed breathing, which unfits the patient for all the active duties of life, more or less in general, but not necessarily, accompanied by cough.

The reader will perceive from all that is said respecting the effects of voltaic electricity in the Second Part of the present volume, the principles which should direct its employment in the cure of disease. The great outline may be comprehended in one sentence. The objects of its employment are for the time to supply the want of nervous influence, and to excite its organs to a better action; the chief precautions to be observed being, to take care that all inflammatory tendency be corrected before its use, and that it shall not excite any such tendency.

It is of course impossible for us to apply this power as nature herself applies it. We can neither confine it correctly to the proper parts, nor correctly apply it in the due degree. With due precaution, however, I cannot help hoping that in those diseases in which the sensorial functions are entire, and the vessels healthy, and merely the functions of assimilation and secretion, which immediately depend on the nervous power, are in fault, voltaic electricity, when the states of the system necessary to secure its beneficial operation are generally understood, will in many instances prove an effectual means of relief in cases where we have few or no others.

As soon as this view of the subject presented itself, I was led to inquire, what diseases depend on a failure of nervous power. The effect on the stomach and lungs, of removing part of the eighth pair of nerves, answered the question respecting two of the most important diseases of this class. The reader has seen, that withdrawing a considerable part of the nervous power from the stomach and lungs deranges the digestive powers, and produces great difficulty of breathing.

When the effect of depriving the lungs of a considerable part of their nervous power is carefully attended to, it will be found in all respects similar to the disease I have termed habitual

asthma: in which the lungs are more or less clogged with phlegm, and the breathing is constantly oppressed, better and worse at different times, but never free, and these symptoms often continue to increase in defiance of every means we can employ, till the patient is permanently unfitted for all the active duties of life.

The animal, in the experiments referred to, is not affected with the croaking noise and violent agitation which generally characterise fits of spasmodic asthma. This state we cannot induce artificially, except by means which lessen the aperture of the windpipe.[1] It is the state of breathing observed in habitual asthma under which it labours.

The reader has seen from repeated trials, that both the oppressed breathing and the collection of phlegm, occasioned by the division of the eighth pair of nerves, may be prevented by causing the voltaic influence to pass through the lungs;[2] and with respect to the stomach, that after its function has been destroyed by depriving it of part of its nervous power, it may be restored by the same influence. That voltaic electricity may be employed with safety in the human body we know from numberless instances, in which it has been applied to it in various ways.

Such are the circumstances which led me to expect relief from it in indigestion and habitual asthma. The first trials were made above twenty years ago, as appears from the paper above referred to. The results in many instances were such as to exceed my expectations, and to have rendered the practice pretty general, although the precautions necessary to render it most successful are, I believe, very little attended to. What these are, I have explained more particularly in my Treatise on Indigestion. For the account of my mode of applying this remedy, I also beg leave to refer the reader to that Treatise.

In it, I have in other respects, as well as the employment of voltaic electricity, applied the results of the foregoing inquiry to

[1] See Chap. ii. [2] Ib.

explain the nature and improve the treatment of indigestion and its consequences; and have been gratified by reports from practitioners in various parts of the kingdom, confirming the advantages derived from the plans of treatment thus suggested; which, with the circumstance of the Treatise having undergone seven editions, encourages me to hope that my labours on this part of the subject have not been in vain.

It is reasonable to suppose, that its having been ascertained that a power which we are at all times capable of commanding, and that to any extent that can be required, having been ascertained to be capable of all the functions of the leading power of the vital system, must, if judiciously employed, often be of eminent service in the practical department of our profession; and such has been the result, although its good effects have been greatly counteracted by little or no attention having generally been paid to the precautions necessary to insure its most beneficial effects.

There is one of its effects, the extent of which in some instances has been very unexpected. It is evident that it may assist the organ to which it is applied in three ways, either by simply supplying the nervous influence where it is defective, in which case its effects are necessarily temporary, an instance of which the reader has seen in its employment in apoplexy; or by exciting the brain and spinal marrow to better action, as far as relates to the diseased part, which better action remains after being once excited, or lastly, by restoring to the ganglionic nerves of the part more vigour in conveying their influence to the part in question. Now I have never, with the exception just mentioned, seen its effects merely temporary, although they are much more so in some cases than in others; therefore its action must always be more or less by one of the last two means; but in some cases so small a power has sometimes produced so great an effect, that it can only be explained by supposing that its operation was wholly in one of these ways.

The following is the most remarkable case of this kind which

has occurred to me. A lady of about thirty years of age applied to me for the removal of permanently oppressed breathing. She had never, even when she was a child, been able to run about with her companions, on account of shortness of breath, as she termed it, for which all remedies had been tried in vain, which, as she advanced in life, so increased, that she could not, when I saw her, even walk up hill without much suffering. I advised no medicine for her, but directed the moderate use of voltaic electricity, which was applied in my presence. In a few minutes she said she breathed with more ease than she remembered ever to have done; but the most unlooked-for part of the result was, that she never required a second application of the remedy, her breath remaining as free as in the most perfect health, so that she could not only walk, but run up hill, with as much ease as other people; and, as far as I know, the effect was permanent, as I never heard anything more of her, which I certainly should have done had the difficulty of breathing returned.

I have seen some similar instances, in which a few applications of voltaic electricity produced permanent relief in the same species of asthma,[1] but none so striking, both from the suddenness of the cure, and because the disease appeared to have been born with the patient.

[1] Voltaic electricity has little if any effect in relieving spasmodic asthma, unless it is complicated with habitual asthma, as I have defined it in the publications referred to, which sometimes happens in protracted cases.

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